REPORT

OF THE

NINTH MEETING

OF THE

BRITISH ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE;

HELD AT BIRMINGHAM IN AUGUST 1839.

LONDON:

JOHN MURRAY, ALBEMARLE STREET.

1840.

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OF

THE ASSOCIATION.

OBJECTS.

THE ASSOCIATION contemplates no interference with the ground occupied by other Institutions. Its objects are,—To give a stronger impulse and a more systematic direction to scientific inquiry,—to promote the intercourse of those who cultivate Science in different parts of the British Empire, with one another, and with foreign philosophers,—to obtain a more general attention to the objects of Science, and a removal of any disadvantages of a public kind which impede its progress.

RULES.

MEMBERS.

All Persons who have attended the first Meeting shall be entitled to become Members of the Association, upon subscribing an obligation to conform to its Rules.

The Fellows and Members of Chartered Literary and Philosophical Societies publishing Transactions, in the British Empire, shall be entitled, in like manner, to become Members of the Association.

The Officers and Members of the Councils, or Managing Committees, of Philosophical Institutions, shall be entitled, in like manner, to become Members of the Association.

All Members of a Philosophical Institution recommended by its Council or Managing Committee, shall be entitled, in like manner, to become Members of the Association.

Persons not belonging to such Institutions shall be elected by the General Committee or Council, to become Members of the Association, subject to the approval of a General Meeting.

SUBSCRIPTIONS.

The amount of the Annual Subscription shall be One Pound, to be paid in advance upon admission; and the amount of the composition in lieu thereof, Five Pounds.

An admission fee of One Pound is required from all Members elected as Annual Subscribers, after the Meeting of 1839, in addition to their annual subscription of One Pound.

Members are entitled to receive copies of any volume of the Transactions for two-thirds of the price at which it is sold to the public; or by one present payment of Five Pounds, as a fixed *Book Subscription*, to receive a copy of all the volumes of Transactions published after the date of such payment.

Subscriptions shall be received by the Treasurer or Secretaries.

If the annual subscription of any Member shall have been in arrear for two years, and shall not be paid on proper notice, he shall cease to be a member.

MEETINGS.

The Association shall meet annually, for one week, or longer. The place of each Meeting shall be appointed by the General Committee at the previous Meeting; and the Arrangements for it shall be entrusted to the Officers of the Association.

GENERAL COMMITTEE.

The General Committee shall sit during the week of the Meeting, or longer, to transact the business of the Association. It shall consist of the following persons:—

1. Presidents and Officers for the present and preceding years, with authors of Reports in the Transactions of the Association.
2. Members who have communicated any Paper to a Philosophical Society, which has been printed in its Transactions, and which relates to such subjects as are taken into consideration at the Sectional Meetings of the Association.
3. Office-bearers for the time being, or Delegates, altogether not exceeding three in number, from any Philosophical Society publishing Transactions.
4. Office-bearers for the time being, or Delegates, not exceeding three, from Philosophical Institutions established in the place of Meeting, or in any place where the Association has formerly met.
5. Foreigners and other individuals whose assistance is desired, and who are specially nominated in writing for the meeting of the year by the President and General Secretaries.
6. The Presidents, Vice-Presidents, and Secretaries of the Sections are *ex officio* members of the General Committee for the time being.

SECTIONAL COMMITTEES.

The General Committee shall appoint, at each Meeting, Committees, consisting severally of the Members most conversant with the several branches of Science, to advise together for the advancement thereof.

The Committees shall report what subjects of investigation they would particularly recommend to be prosecuted during the ensuing year, and brought under consideration at the next Meeting.

The Committees shall recommend Reports on the state and progress of particular Sciences, to be drawn up from time to time by competent persons, for the information of the Annual Meetings.

COMMITTEE OF RECOMMENDATIONS.

The General Committee shall appoint at each Meeting a Committee, which shall receive and consider the Recommendations of the Sectional Committees, and report to the General Committee the measures which they would advise to be adopted for the advancement of Science.

All Recommendations of Grants of Money, Requests for Special Researches, and Reports on Scientific Subjects, shall be submitted to the Committee of Recommendations, and not taken into consideration by the General Committee, unless previously recommended by the Committee of Recommendations.

LOCAL COMMITTEES.

Local Committees shall be formed by the Officers of the Association to assist in making arrangements for the Meetings.

Committees shall have the power of adding to their numbers those Members of the Association whose assistance they may desire.

OFFICERS.

A President, two or more Vice-Presidents, one or more Secretaries, and a Treasurer, shall be annually appointed by the General Committee.

COUNCIL.

In the intervals of the Meetings, the affairs of the Association shall be managed by a Council appointed by the General Committee. The Council may also assemble for the despatch of business during the week of the Meeting.

PAPERS AND COMMUNICATIONS.

The Author of any paper or communication shall be at liberty to reserve his right of property therein.

ACCOUNTS.

The Accounts of the Association shall be audited annually, by Auditors appointed by the Meeting.

OFFICERS AND COUNCIL, 1839–40.

Trustees (permanent.)—Francis Baily, Esq. R. I. Murchison, Esq. John Taylor, Esq.

President.—The Rev. William Vernon Harcourt, F.R.S. G.S.

Vice-Presidents.—The Marquis of Northampton. The Earl of Dartmouth. The Rev. T. R. Robinson, D.D. John Corrie, Esq., *deceased.*

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Vice-Presidents elect.—The very Rev. Principal Macfarlane. Major-Gen. Lord Greenock. Sir David Brewster. Sir Thos. Macdougall Brisbane.

General Secretaries.—R. I. Murchison, Esq., F.R.S. Major Sabine, F.R.S.

Assistant General Secretary.—John Phillips, Esq., F.R.S. *York.*

Secretaries for Glasgow.—Rev. J. P. Nicol, LL.D. Andrew Liddell, Esq. John Strang, Esq.

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Council.—Dr. Arnott. F. Baily, Esq. R. Brown, Esq. Rev. Dr. Buckland. The Earl of Burlington. Professor Daniell. Dr. Daubeny. Professor T. Graham. J. E. Gray, Esq. G. B. Greenough, Esq. Dr. Hodgkin. R. Hutton, Esq. M.P. Dr. Lardner. Dr. R. Lee. Sir C. Lemon, Bart. J. W. Lubbock, Esq. C. Lyell, Esq. Professor Moseley. Professor Owen. The Very Rev. Dr. Peacock. Professor Powell. George Rennie, Esq. Lieut.-Col. Sykes. Captain Washington. Professor Wheatstone. Professor Whewell.

Secretary to the Council.—James Yates, Esq., F.R.S. 49, Upper Bedford Place, London.

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I. Table showing the Places and Times of Meeting of the British Association, with Presidents, Vice-Presidents, and Local Secretaries, from its Commencement.

Presidents.	Vice-Presidents.	Local Secretaries.
The EARL FITZWILLIAM, D.C.L., F.R.S., F.G.S., &c. YORK, September 27, 1831.	Rev. W. Vernon Harcourt, M.A., F.R.S., F.G.S.	William Gray, jun., F.G.S. Professor Phillips, F.R.S., F.G.S.
The REV. W. BUCKLAND, D.D., F.R.S., F.G.S., &c. OXFORD, June 19, 1832.	Sir David Brewster, F.R.SS.L. & E., &c. Rev. W. Whewell, F.R.S., Pres. Geol. Soc.	Professor Daubeny, M.D., F.R.S., &c. Rev. Professor Powell, M.A., F.R.S., &c.
The REV. ADAM SEDGWICK, M.A., V.P.R.S., V.P.G.S. CAMBRIDGE, June 25, 1833.	G. B. Airy, F.R.S., Astronomer Royal, &c. John Dalton, D.C.L., F.R.S.	Rev. Professor Henslow, M.A., F.L.S., F.G.S. Rev. Wm. Whewell, F.R.S.
Sir T. MACDOUGAL BRISBANE, K.C.B., D.C.L., F.R.SS.L. & E. EDINBURGH, September 8, 1834.	Sir David Brewster, F.R.S., &c. Rev. T. R. Robinson, D.D.	Professor Forbes, F.R.SS.L. & E., &c. Sir John Robison, Sec. R.S.E.
The REV. PROVOST LLOYD, LL.D. DUBLIN, August 10, 1835.	Viscount Oxmantown, F.R.S., F.R.A.S. Rev. W. Whewell, F.R.S., &c.	Sir W. R. Hamilton, Astron. Royal of Ireland, &c. Rev. Professor Lloyd, F.R.S.
The MARQUIS OF LANSDOWNE, D.C.L., F.R.S., &c. BRISTOL, August 22, 1836.	The Marquis of Northampton, F.R.S. Rev. W. D. Conybeare, F.R.S., F.G.S. J. C. Prichard, M.D., F.R.S.	Professor Daubeny, M.D., F.R.S., &c. V. F. Hovenden.
The EARL OF BURLINGTON, F.R.S., F.G.S., Chan. Univ. Lon. LIVERPOOL, September 11, 1837.	The Bishop of Norwich, P.L.S., F.G.S. John Dalton, D.C.L., F.R.S. Sir Philip Grey Egerton, Bart., F.R.S., F.G.S. Rev. W. Whewell, F.R.S.	Professor Traill, M.D. Wm. Wallace Currie. Joseph N. Walker, Pres. Royal Institution, Liverpool.
The DUKE OF NORTHUMBERLAND, F.R.S., F.G.S., &c. NEWCASTLE-ON-TYNE, August 20, 1838.	The Bishop of Durham, F.R.S., F.S.A. The Rev. W. Vernon Harcourt, F.R.S., &c. Prideaux John Selby, Esq., F.R.S.E.	John Adamson, F.L.S., &c. Wm. Hutton, F.G.S. Professor Johnston, M.A., F.R.S.
The REV. W. VERNON HARCOURT, M.A. BIRMINGHAM, August 26, 1839.	The Marquis of Northampton The Earl of Dartmouth The Rev. T. R. Robinson, D.D. John Corrie, Esq., F.R.S.	George Barker, Esq., F.R.S. Peyton Blakiston, M.D. Joseph Hodgson, Esq., F.R.S. Follett Osler, Esq.
The MOST NOBLE THE MARQUIS OF BREADALBANE. GLASGOW, September 17, 1840.	Very Rev. Principal Macfarlane Major-General Lord Greenock, F.R.S.E. Sir David Brewster, F.R.S. Sir T. M. Brisbane, Bart. F.R.S.	Andrew Liddell, Esq. Rev. J. P. Nicol, LL.D. John Strang, Esq.

II. Table showing the Members of Council of the British Association from its Commencement, in addition to Presidents, Vice-Presidents, and Local Secretaries.

General Secretaries.	Rev. Wm. Vernon Harcourt, F.R.S., &c. 1832—1836. Francis Baily, V.P. and Treas. R.S. ......1835. R. I. Murchison, F.R.S., F.G.S. ....1836—1839. Rev. G. Peacock, F.R.S., F.G.S., &c. ...1837, 1838.
General Treasurer.	John Taylor, F.R.S., Treas. G.S., &c. ...1832—1839.
Trustees (permanent).	Charles Babbage, F.R.SS.L. & E., &c. (Resigned.) R. I. Murchison, F.R.S., &c. John Taylor, F.R.S., &c. Francis Baily, F.R.S.
Assistant General Secretary.	Professor Phillips, F.R.S., &c. ....1832—1839.

Members of Council.

G. B. Airy, F.R.S., Astronomer Royal ......1834, 1835.
Neill Arnott, M.D. ....1838, 1839.
Francis Baily, V.P. and Treas. R.S. ....1837—1839.
George Bentham, F.L.S. ....1834, 1835.
Robert Brown, D.C.L., F.R.S. ....1832, 1834, 1835, 1838, 1839.
Sir David Brewster, F.R.S., &c. ....1832.
M. I. Brunel, F.R.S., &c. ....1832.
Rev. Professor Buckland, D.D., F.R.S., &c. .1833, 1835, 1838, 1839.
The Earl of Burlington .... 1838, 1839.
Rev. T. Chalmers, D.D., Prof. of Divinity, Edinburgh ....1833.
Professor Clark, Cambridge ....1838.
Professor Christie, F.R.S., &c...................1833—1837.
William Clift, F.R.S., F.G.S. ....1832—1835.
John Corrie, F.R.S., &c. ....1832.
Professor Daniell, F.R.S. ....1836, 1839.
Dr. Daubeny............. ....1838, 1839.
J. E. Drinkwater ....1834, 1835.
The Earl Fitzwilliam, D.C.L., F.R.S., &c....1833.
Professor Forbes, F.R.SS.L. & E., &c. ......1832.
Davies Gilbert, D.C.L., V.P.R.S., &c. ......1832.
Professor R. Graham, M.D., F.R.S.E.........1837.
Professor Thomas Graham, F.R.S. ....1838, 1839.
John Edward Gray, F.R.S., F.L.S., &c.......1837—1839.
Professor Green, F.R.S., F.G.S. ....1832.
G. B. Greenough, F.R.S., F.G.S. ....1832—1839.
Henry Hallam, F.R.S., F.S.A., &c............1836.
Sir William R. Hamilton, Astron. Royal of Ireland ....1832, 1833, 1836.
Rev. Prof. Henslow, M.A., F.L.S., F.G.S....1837.
Sir John F. W. Herschel, F.R.SS. L. &. E., F.R.A.S., F.G.S., &c. ....1832.
Thomas Hodgkin, M.D. ....1833—1837, 1839.
Prof. Sir W. J. Hooker, LL.D., F.R.S., &c. .1832.
Rev. F. W. Hope, M.A., F.L.S.................1837.
Robert Hutton, M.P., F.G.S., &c. ....1836, 1838, 1839.
Professor R. Jameson, F.R.SS. L. & E. ......1833.

Rev. Leonard Jenyns ....1838.
Dr. R. Lee..1839.
Sir C. Lemon, Bart., M.P. ....1838, 1839.
Rev. Dr. Lardner ....1838, 1839.
Professor Lindley, F.R.S., F.L.S., &c. ......1833, 1836.
Rev. Provost Lloyd, D.D. ....1832, 1833.
J. W. Lubbock, F.R.S., F.L.S., &c., Vice-
Chancellor of the University of London1833—1836, 1838, 1839.
Rev. Thomas Luby ....1832.
Charles Lyell, jun., Esq. ....1838, 1839.
William Sharp MacLeay, F.L.S................1837.
Professor Moseley.................................1839.
Patrick Neill, LL.D., F.R.S.E. ....1833.
Richard Owen, F.R.S., F.L.S....................1836, 1838, 1839.
Rev. George Peacock, M.A., F.R.S., &c. ...1832, 1834, 1835, 1839.
Rev. Professor Powell, M.A., F.R.S., &c. ...1836, 1837, 1839.
J. C. Prichard, M.D., F.R.S., &c. ....1832.
George Rennie, F.R.S............................1833—1835, 1839.
Sir John Rennie ....1838.
Rev. Professor Ritchie, F.R.S. ....1833.
Sir John Robison, Sec. R.S.E...................1832, 1836.
P. M. Roget, M.D., Sec. R.S., F.G.S., &c....1834—1837.
Major Sabine.......................................1838.
Rev. William Scoresby, B.D., F.R.SS. L.&E.1832.
Lieut.-Col. W. H. Sykes, F.R.S., F.L.S., &c.1837—1839.
Rev. J. J. Tayler, B.A., Manchester ....1832.
Professor Traill, M.D. ....1832, 1833.
N. A. Vigors, M.P., D.C.L., F.S.A., F.L.S.1832, 1836.
Captain Washington, R.N. ....1838, 1839.
Professor Wheatstone ....1838, 1839.
Rev. W. Whewell... ....1838, 1839.
William Yarrell, F.L.S. ....1833—1836.

Secretaries to the Council. { Edward Turner, M.D., F.R.SS. L. & E...1832—1836
James Yates, F.R.S., F.L.S., F.G.S.......1832—1839

OFFICERS OF SECTIONAL COMMITTEES AT THE BIRMINGHAM MEETING.

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE.

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TREASURER'S ACCOUNT from

RECEIPTS.

	£	s.	d.	£	s.	d.
Balance in hand from last year's Account				670	0	7
Compositions from 132 Members, Newcastle and since	641	0	0			
Subscriptions, 1838, from 1944 Members, do.	1944	0	0			
Ditto 1839, from 37 do. do.	37	0	0			
Arrears 1837, from 37 do. do.	37	1	0			
				2659	1	0
Dividend on £5500 in 3 per cent. consols, 12 months to July last				165	0	0
Received on account of Sale of Reports, viz.						
1st vol., 2nd Edition	27	7	2			
2nd vol.	28	10	4			
3rd vol.	37	13	0			
4th vol.	39	18	6			
5th vol.	44	4	4			
6th vol.	233	5	8			
Lithographs sold	1	10	6			
				412	9	6
				£3906	11	1

1st August 1838 to 15th August 1839 inclusive.

PAYMENTS.

			£ s. d.
Expenses of Meeting at Newcastle, allowed by Order of the Council......			500 0 0
Disbursements by Local Treasurers...			156 18 8
Salaries to Assistant Secretary and Accountant, 12 months to Midsummer			250 0 0
Grants to Committees for Scientific purposes, viz. for			
Reduction of Stars in Histoire Céleste	1837 21 18 6 1838 150 0 0	171 18 6	
Do. do. Lacaille		11 0 0	
Catalogue of Stars, 1837		166 16 6	
Land and Sea Level, 1838...		52 1 4	
Do. do. 1837...		222 0 0	
Tides' Discussions at Bristol.......................................		35 18 6	
Mechanism of Waves, 1837		94 2 0	
Do. do. 1838		50 0 0	
Meteorological Observations, Plymouth	40 0 0	55 0 0	
Do. do. hourly, Scotland	15 0 0		
Completing Anemometer		8 10 0	
Meteorology and Subterranean Temp. 1837, Thermometers		21 11 0	
Atmospheric Air..		16 1 0	
Action of Sea Water on Iron	1837 20 0 0 1838 20 0 0	40 0 0	
Action of Hot Water on Organic Bodies.........................		3 0 0	
British Fossil Ichthyology	1837 5 0 0 1838 105 0 0	110 0 0	
Fossil Reptiles...		118 2 9	
Mining Statistics		50 0 0	
Duty of Cornish Steam Engines		50 0 0	
Marine Steam Engines		100 0 0	
Experiments on Vitrification (old grant)........................		9 4 7	
Do. on Strength of Iron..................................		100 0 0	
Animal Secretions, 1837		10 10 0	
Gases on Solar Spectrum, Action of..............................		22 0 0	
Railway Constants 1837	8 7 2	28 7 2	
Do. 1838	20 0 0		
			1595 11 0
Paid for Printing Reports, 6th vol.		629 14 2	
Do. for Engravings for do.		171 19 10	
			801 14 0
Printing List of Members..			77 12 0
Sundry Printing, Advertising, &c.			38 14 6
Sundry Expenses on Publishing Reports			25 7 7
Balance in the hands of the Bankers		352 15 0	
Do. Treasurer and Local Treasurers		107 18 4	
			460 13 4
			£3906 11 1

The following Reports on the Progress and Desiderata of different branches of Science have been drawn up at the request of the Association, and printed in its Transactions.

1831–2.

On the progress of Astronomy during the present century, by G. B. Airy, M.A., Astronomer Royal.

On the state of our knowledge respecting Tides, by J. W. Lubbock, M.A., Vice-President of the Royal Society.

On the recent progress and present state of Meteorology, by James D. Forbes, F.R.S., Professor of Natural Philosophy, Edinburgh.

On the present state of our knowledge of the Science of Radiant Heat, by the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry, Oxford.

On Thermo-electricity, by the Rev. James Cumming, M.A., F.R.S., Professor of Chemistry, Cambridge.

On the recent progress of Optics, by Sir David Brewster, K.C.G., LL.D., F.R.S., &c.

On the recent progress and present state of Mineralogy, by the Rev. William Whewell, M.A. F.R.S.

On the progress, actual state, and ulterior prospects of Geology, by the Rev. William Conybeare, M.A., F.R.S., V.P.G.S., &c.

On the recent progress and present state of Chemical Science, by J. F. W. Johnston, A.M., Professor of Chemistry, Durham.

On the application of Philological and Physical researches to the History of the Human species, by J. C. Prichard, M.D., F.R.S., &c.

1833.

On the advances which have recently been made in certain branches of Analysis, by the Rev. G. Peacock, M.A., F.R.S., &c.

On the present state of the Analytical Theory of Hydrostatics and Hydrodynamics, by the Rev. John Challis, M.A. F.R.S., &c.

On the state of our knowledge of Hydraulics, considered as a branch of Engineering, by George Rennie, F.R.S., &c. (Parts I. and II.)

On the state of our knowledge respecting the Magnetism of the Earth, by S. H. Christie, M.A., F.R.S., Professor of Mathematics, Woolwich.

On the state of our knowledge of the Strength of Materials, by Peter Barlow, F.R.S.

On the state of our knowledge respecting Mineral Veins, by John Taylor, F.R.S., Treasurer G.S., &c.

On the state of the Physiology of the Nervous System, by William Charles Henry, M.D.

On the recent progress of Physiological Botany, by John Lindley, F.R.S., Professor of Botany in the University of London.

1834.

On the Geology of North America, by H. D. Rogers, F.G.S.

On the philosophy of Contagion, by W. Henry, M.D., F.R.S.

On the state of Physiological Knowledge, by the Rev. Wm. Clark, M.D., F.G.S., Professor of Anatomy, Cambridge.

On the state and progress of Zoology, by the Rev. Leonard Jenyns, M.A., F.L.S., &c.

On the theories of Capillary Attraction, and of the Propagation of Sound as affected by the Development of Heat, by the Rev. John Challis, M.A., F.R.S., &c.

On the state of the science of Physical Optics, by the Rev. H. Lloyd, M.A., Professor of Natural Philosophy, Dublin.

1835.

On the state of our knowledge respecting the application of Mathematical and Dynamical principles to Magnetism, Electricity, Heat, &c., by the Rev. Wm. Whewell, M.A., F.R.S.

On Hansteen's researches in Magnetism, by Captain Sabine, F.R.S.

On the state of Mathematical and Physical Science in Belgium, by M. Quetelet, Director of the Observatory, Brussels.

1836.

On the present state of our knowledge with respect to Mineral and Thermal Waters, by Charles Daubeny, M.D., F.R.S., M.R.I.A., &c., Professor of Chemistry and of Botany, Oxford.

On North American Zoology, by John Richardson, M.D., F.R.S., &c.

Supplementary Report on the Mathematical Theory of Fluids, by the Rev. J. Challis, Plumian Professor of Astronomy in the University of Cambridge.

1837.

On the variations of the Magnetic Intensity observed at different points of the Earth's Surface, by Major Edward Sabine, R.A., F.R.S.

On the various modes of Printing for the use of the Blind, by the Rev. William Taylor, F.R.S.

On the present state of our knowledge in regard to Dimorphous Bodies, by Professor Johnston, F.R.S.

On the Statistics of the Four Collectorates of Dukhun, under the British Government, by Col. Sykes, F.R.S.

1838.

Appendix to Report on the variations of Magnetic Intensity, by Major Edward Sabine, R.A., F.R.S.

The following Reports of Researches undertaken at the request of the Association have been published, viz.

1835.

On the comparative measurement of the Aberdeen Standard Scale, by Francis Baily, Treasurer R.S., &c.

On Impact upon Beams, by Eaton Hodgkinson.

Observations on the Direction and Intensity of the Terrestrial Magnetic Force in Ireland, by the Rev. H. Lloyd, Capt. Sabine, and Capt. J. C. Ross.

On the Phænomena usually referred to the Radiation of Heat, by H. Hudson, M.D.

Experiments on Rain at different Elevations, by Wm. Gray, jun., and Professor Phillips.

Hourly observations of the Thermometer at Plymouth, by W. S. Harris.

On the Infra-orbital Cavities in Deers and Antelopes, by A. Jacob, M.D.

On the Effects of Acrid Poisons, by T. Hodgkin, M.D.

On the Motions and Sounds of the Heart, by the Dublin Sub-Committee.

On the Registration of Deaths, by the Edinburgh Sub-Committee.

1836.

Observations on the Direction and Intensity of the Terrestrial Magnetic Force in Scotland, by Major Edward Sabine, R.A., F.R.S., &c.

Comparative view of the more remarkable Plants which characterize the Neighbourhood of Dublin, the Neighbourhood of Edinburgh, and the South-west of Scotland, &c.; drawn up for the British Association, by J. T. Mackay, M.R.I.A., A.L.S., &c., assisted by Robert Graham, Esq., M.D., Professor of Botany in the University of Edinburgh.

Report of the London Sub-Committee of the Medical Section of the British Association on the Motions and Sounds of the Heart.

Second Report of the Dublin Sub-Committee on the Motions and Sounds of the Heart. (See Vol. iv. p. 243.)

Report of the Dublin Committee on the Pathology of the Brain and Nervous System.

Account of the Recent Discussions of Observations of the Tides which have been obtained by means of the grant of money which was placed at the disposal of the Author for that purpose at the last Meeting of the Association, by J. W. Lubbock, Esq.

Observations for determining the Refractive Indices for the Standard Rays of the Solar Spectrum in various media, by the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry in the University of Oxford.

Provisional Report on the Communication between the Arteries and Absorbents on the part of the London Committee, by Dr. Hodgkin.

Report of Experiments on Subterranean Temperature, under the direction of a Committee, consisting of Professor Forbes, Mr. W. S. Harris, Professor Powell, Lieut.-Colonel Sykes, and Professor Phillips (Reporter).

Inquiry into the validity of a method recently proposed by George B. Jerrard, Esq., for Transforming and Resolving Equations of Elevated Degrees: undertaken at the request of the Association by Professor Sir W. R. Hamilton.

1837.

Account of the Discussions of Observations of the Tides which have been obtained by means of the grant of money which was placed at the disposal of the Author for that purpose at the last Meeting of the Association, by J. W. Lubbock, Esq., F.R.S.

On the difference between the Composition of Cast Iron produced by the Cold and the Hot Blast, by Thomas Thomson, M.D., F.R.SS. L. & E., &c., Professor of Chemistry, Glasgow.

On the Determination of the Constant of Nutation by the Greenwich Observations, made as commanded by the British Association, by the Rev. T. R. Robinson, D.D.

On some Experiments on the Electricity of Metallic Veins, and the Temperature of Mines, by Robert Were Fox.

Provisional Report of the Committee of the Medical Section of the British Association, appointed to investigate the Composition of Secretions, and the Organs producing them.

Report from the Committee for inquiring into the Analysis of the Glands, &c. of the Human Body, by G. O. Rees, M.D., F.G.S.

Second Report of the London Sub-Committee of the British Association Medical Section, on the Motions and Sounds of the Heart.

Report from the Committee for making experiments on the Growth of Plants under Glass, and without any free communication with the outward air, on the plan of Mr. N. I. Ward, of London.

Report of the Committee on Waves, appointed by the British

Association at Bristol in 1836, and consisting of Sir John Robison, K.H., Secretary of the Royal Society of Edinburgh, and John Scott Russell, Esq., M.A., F.R.S. Edin. (Reporter).

On the relative Strength and other mechanical Properties of Cast Iron obtained by Hot and Cold Blast, by Eaton Hodgkinson.

On the Strength and other Properties of Iron obtained from the Hot and Cold Blast, by W. Fairbairn.

1838.

Account of a Level Line, measured from the Bristol Channel to the English Channel, during the Year 1837–8, by Mr. Bunt, under the Direction of a Committee of the British Association. Drawn up by the Rev. W. Whewell, F.R.S., one of the Committee.

A Memoir on the Magnetic Isoclinal and Isodynamic Lines in the British Islands, from Observations by Professors Humphrey Lloyd and John Phillips, Robert Were Fox, Esq., Captain James Clark Ross, R.N., and Major Edward Sabine, R.A., by Major Edward Sabine, R.A., F.R.S.

First Report on the Determination of the Mean Numerical Values of Railway Constants, by Dionysius Lardner, LL.D., F.R.S., &c.

First Report upon Experiments, instituted at the request of the British Association, upon the Action of Sea and River Water, whether clear or foul, and at various temperatures, upon Cast and Wrought Iron, by Robert Mallet, M.R.I.A., Ass. Ins. C.E.

Notice of Experiments in progress, at the desire of the British Association, on the Action of a Heat of 212° Fahr., when long continued, on Inorganic and Organic Substances, by Robert Mallet, M.R.I.A.

Experiments on the ultimate Transverse Strength of Cast Iron made at Arigna Works, Co. Leitrim, Ireland, at Messrs. Bramah and Robinson's, 29th May, 1837.

Provisional Reports and Notices of Progress in Special Researches entrusted to Committees and Individuals.

1839.

Report on the present state of our knowledge of Refractive Indices for the Standard Rays of the Solar Spectrum in different media. By the Rev. Baden Powell, M.A., F.R.S., F.G.S., F.R.Ast.S., Savilian Professor of Geometry, Oxford.

Report on the application of the sum assigned for Tide Calculations to Mr. Whewell, in a Letter from T. G. Bunt, Esq., Bristol.

Notice of Determination of the Arc of Longitude between the Observatories of Armagh and Dublin. By the Rev. T. R. Robinson, D.D., &c.

Report of some Galvanic Experiments to determine the existence or non-existence of Electrical Currents among Stratified Rocks, particularly those of the Mountain Limestone formation, constituting the Lead Measures of Alston Moor. By H. L. Pattinson, Esq.

Report respecting the two series of Hourly Meteorological Observations kept in Scotland at the expense of the British Association. By Sir David Brewster, K.H., LL.D., F.R.S. L. and E.

Report on the subject of a series of Resolutions adopted by the British Association at their Meeting in August, 1838, at Newcastle.

Report on British Fossil Reptiles. By Richard Owen, Esq., F.R.S., F.G.S., &c.

Report on the distribution of Pulmoniferous Mollusca in the British Isles. By Edward Forbes, M.W.S., For. Sec. B.S.

Third Report on the Progress of the Hourly Meteorological Register at the Plymouth Dock-yard, Devonport. By W. Snow Harris, Esq., F.R.S.

The following Reports and Continuations of Reports have been undertaken to be drawn up at the request of the Association.

On the state of knowledge of the Phænomena of Sound, by Rev. Robert Willis, M.A., F.R.S., &c.

On the state of our knowledge respecting the relative level of Land and Sea, and the waste and extension of the land on the east coast of England, by R. Stevenson, Engineer to the Northern Lighthouses, Edinburgh.

On Salts, by Professor Graham, F.R.S.

On the Differential and Integral Calculus, by Rev. Professor Peacock, M.A., F.R.S., &c.

On the Geology of North America, by H. D. Rogers, F.G.S., Professor of Geology, Philadelphia.

On Vision, by Professor C. Wheatstone, F.R.S.

On the application of a General Principle in Dynamics to the Theory of the Moon, by Professor Sir W. Hamilton.

On Isomeric Bodies, by Professor Liebig.

On Organic Chemistry, by Professor Liebig.

On Inorganic Chemistry, by Professor Johnston, F.R.S.

On Fossil Reptiles (continuation), by Professor Owen, F.R.S.

On the Salmonidæ of Scotland, by Sir J. W. Jardine.

On the Caprimulgidæ, by N. Gould, F.L.S.

On the state of Meteorology in the United States of North America, by A. Bache.

On the state of Chemistry as bearing on Geology, by Professor Johnston.

On Molluscous Animals and their Shells, by J. E. Gray, F.R.S.

On Ornithology, by P. J. Selby, F.R.S.E.

On the Specific Gravity of Steam, by a Committee, of which Mr. B. Donkin is Secretary.

Recommendations for Additional Reports, Researches, and Grants of Money sanctioned by the General Committee at the Birmingham Meeting.

ADDITIONAL REPORTS ON THE STATE OF SCIENCE REQUESTED.

That Mr. W. J. Henwood be requested to furnish a Report of his own observations on the Temperature of the deep Mines of Cornwall.

That Mr. R. W. Fox be requested to furnish a Report of his own observations on Subterranean Temperature.

That Professor Miller be requested to furnish a Report on the recent progress and present state of the Science of Crystallography.

That Professor Forbes be requested to furnish a supplementary Report on Meteorology.

That Professor Powell be requested to furnish a supplementary Report on Radiant Heat.

That Professor de la Rive, of Geneva, be requested to furnish a Report on the recent progress and present condition of Electro-Chemistry and Electro-Magnetism.

Additional Recommendations involving applications to Government or Public Bodies sanctioned by the General Committee at the Birmingham Meeting.

GEOLOGY.

That with a view to supply one of the greatest desiderata at present felt by geologists in investigating the structure and history of the earth, as well as to advance a branch of geology,

for the study of which no adequate provision has hitherto been made in any of the public institutions of this country, application be made to the trustees of the British Museum to form a conchological collection, if possible, under the same roof, and which may include not only all known species of shells, whether recent or fossil, but likewise the varieties of form and size which such species assume at different periods of their growth, or from other causes, together with a series of the impressions of shells which are found upon different rocks and plaster casts from their impressions; and that the Marquis of Northampton be requested to bring this recommendation before the Board of Trustees.

MINING RECORDS.

The Committee appointed at Newcastle, for the purpose of applying to Government for a proper place for the deposit of records connected with the mining transactions of Great Britain, having reported that a room adjoining the Museum of Economic Geology had been appointed for their reception, which would also be placed under the custody of a proper person, it was resolved,—That the following gentlemen should be appointed a Committee for the purpose of superintending and making the necessary arrangements, and for assisting in the collection and transmission of Mining Records:—Marquis of Northampton, Sir Charles Lemon, Sir Philip G. Egerton, John Henry Vivian, Esq., Davies Gilbert, Esq., J. S. Enys, Esq., W. L. Dillwyn, Esq., Charles Lyell, Esq., the President of the Geological Society, the Professors of Geology at the Universities of Oxford, Cambridge, London, and Durham, H. T. De la Beche, Esq., John Buddle, Esq., Thos. Sopwith, Esq., Richard Griffith, Esq., James Barker, Esq., the President of the British Association, G. R. Porter, Esq., John Taylor, Esq.

Synopsis of Sums appropriated to Scientific Objects by the General Committee at the Birmingham Meeting.

SECTION A.

1. For the Reduction of Meteorological Observations, under the superintendence of Sir J. Herschel	£100	0	0
2. For the Reduction of Lacaille's Stars, under the superintendence of Sir J. Herschel, the Astronomer Royal, and Mr. Henderson	189	0	0
3. For the Revision of the Nomenclature of the Stars: Sir John Herschel, Mr. Whewell, and Mr. Baily	50	0	0
4. For the Reduction of Stars in the *Histoire Céleste*: Mr. Baily, the Astronomer Royal, and Dr. Robinson	328	1	6
5. To extend the Royal Astronomical Society's Catalogue: Mr. Baily, the Astronomer Royal, and Dr. Robinson	343	3	6
6. For Magnetical Observations (Instruments, &c.): Sir J. Herschel, Mr. Whewell, Dr. Peacock, Mr. Lloyd, and Major Sabine	400	0	0
7. Hourly Meteorological Observations in Scotland: Sir D. Brewster and Mr. Forbes	50	12	4
8. To the Committee on Waves: Sir J. Robison and Mr. J. S. Russell	30	0	0
*9. For the Reduction and Tabulation of Observations on Subterranean Temperature: under the superintendence of Prof. Forbes	20	0	0
10. For Tide Discussions: Mr. Whewell	50	0	0
*11. For procuring an Engraved Plate for tabulating observations: under the direction of Prof. Forbes	10	0	0
12. For the Translation of Foreign Scientific Memoirs: Major Sabine, Dr. R. Brown, Dr. Robinson, Sir J. Herschel, and Prof. Wheatstone. (In the application for this grant the Committee of Section D. also joined.)	100	0	0
*13 For Observations with Prof. Whewell's Anemometer at Plymouth: under the superintendence of Mr. Snow Harris	10	0	0
Carried forward	£1680	17	4

Brought forward	£1680	17	4
*14 For Alterations and Observations with Mr. Osler's Anemometer at Plymouth: under the superintendence of Mr. Snow Harris .	30	0	0
15. For the Expenses of Meteorological Observations at Plymouth (additional grant): Mr. W. S. Harris	40	0	0
*16. For procuring and fixing an Anemometer on Mr. Osler's construction, to be placed at some station in Scotland: under the superintendence of Prof. Forbes	60	0	0
	£1810	17	4

Section B.

17. For Researches on Atmospheric Air: Mr. W. West	24	0	0
18. For Experiments on the Action of Sea Water on Cast and Wrought Iron: Mr. Mallet and Prof. Davy	30	0	0
19. For Experiments on the Action of Water of 212° on Organic Matter: Mr. Mallet . .	7	0	0
20. For Experiments on the Specific Gravity of the Gases: Dr. Prout, and Prof. Clark of Aberdeen	40	0	0
*21. For defraying the expenses of certain Experiments by Prof. Schönbein, of Basle, on the connexion between Chemical and Electrical Phænomena: the result to be reported to the Association at their next Meeting .	40	0	0
	£141	0	0

Section C.

22. For the Promotion of our Knowledge of British Fossil Reptiles, by a Report on that subject: Mr. Greenough, Mr. Lyell, and Mr. Clift	81	17	3
	£81	17	3

Section D.

23. For Experiments on the Preservation of Animal and Vegetable Substances: Prof.

Henslow, Mr. Jenyns, Dr. Clark, and Prof. Cumming	£6	0	0
*24. For Procuring Drawings, illustrative of the Species and their Details, of the Radiate Animals of the British Islands, to accompany a Report of the State of our Knowledge of such Animals: under the superintendence of Mr. Gray, Mr. Forbes, Mr. Goodsir, Mr. Patterson, Mr. Thompson of Belfast, and Dr. George Johnston . . .	50	0	0
*25. For Researches with the Dredge, with a view to the investigation of the Marine Zoology of Great Britain, the Illustration of the Geographical Distribution of Marine Animals, and the more accurate determination of the Fossils of the Pleiocene Period: under the superintendence of Mr. Gray, Mr. Forbes, Mr. Goodsir, Mr. Patterson, Mr. Thompson of Belfast, Mr. Ball of Dublin, Dr. George Johnston, Mr. Smith of Jordan Hill, and Mr. A. Strickland	60	0	0
*26. For the Engraving of Skeleton Maps for recording the Distribution of Plants and Animals: under the superintendence of Prof. R. Graham, Dr. Greville, Mr. Brand, Mr. H. Watson, Mr. J. E. Gray, and Mr. E. Forbes	20	0	0
*27. For Printing and Circulating a Series of Questions and Suggestions for the use of travellers and others, with a view to procure Information respecting the different races of Men, and more especially of those which are in an uncivilized state: the Questions to be drawn up by Dr. Prichard, Dr. Hodgkin, Mr. J. Yates, Mr. Gray, Mr. Darwin, Mr. R. Taylor, Dr. Wiseman, and Mr. Yarrell	5	0	0
	£141	0	0

SECTION E.

28. For Experiments on the Sounds of the Heart: Dr. Clendinning	25	0	0
Carried forward	£25	0	0

Brought forward	£25	0	0
29. For Experiments on the Lungs and Bronchi: Dr. C. Williams	25	0	0
30. For Experiments on Medico-Acoustic Instruments: Dr. Yelloly	25	0	0
31. For Investigations on the Veins and Absorbents: Dr. Roget	25	0	0
32. For Experiments on Acrid Poisons: Dr. Roupell	25	0	0
	£125	0	0

Section F.

33. For Statistical Inquiries in Schools for the Working Classes: under the superintendence of Sir Charles Lemon, Mr. Hallam, and Mr. G. R. Porter	100	0	0
	£100	0	0

Section G.

*34. For Inquiries respecting the Duty of American Steam Boats: under the direction of Mr. W. Fairbairn, Dr. Lardner, Mr. I. S. Russell, Mr. John Taylor, and Mr. Allen of New York	50	0	0
35. For Inquiries respecting the Duty of Engines not in Cornwall: Mr. W. Bryan Donkin, Mr. James Simpson, Mr. G. H. Palmer, and Mr. T. Webster, Sec. . . .	20	0	0
36. For Experiments on the Hot-blast Iron as compared with Cold-blast Iron: Mr. Hodgkinson, Mr. W. Fairbairn, and Mr. P. Clare	100	0	0
*37 For Experiments on the Increase, after long periods, in the Deflexion of Beams and other Structures, variously loaded: under the superintendence of Mr. George Cottam, Mr. J. Glynn, and Mr. T. Edginton . .	20	0	0
38. For Experiments on the Forms of Vessels: Sir J. Robison, Mr. I. S. Russell, and Mr. James Smith	200	0	0
	390	0	0

Section	A.	£1810	17	4
—	B.	141	0	0
—	C.	81	17	3
—	D.	141	0	0
—	E.	125	0	0
—	F.	100	0	0
—	G.	390	0	0
		2789	14	7

The above grants expire at the Meeting in 1840, unless the Recommendations shall have been acted on, or a continuance of the grant applied for by the Sectional Committees, and ordered by the General Committee. Those marked thus *, relating to subjects on which no previous resolution has been adopted, are generally explained at greater length than the others, which are renewals or continuations of former grants for objects which have been detailed in previous volumes.

In grants of money to Committees for purposes of Science, the member first named is empowered to draw on the Treasurer for such sums as may from time to time be required. The General Committee does not contemplate, in the grants, the payment of personal expenses to the members.

On Monday evening, August 29th, the President, the Rev. W. V. Harcourt, took the Chair in the Town Hall, and delivered an Address to the Meeting (see next page).

On Saturday evening, the Concluding General Meeting of the Association took place in the Town Hall, when an account of the Proceedings of the General Committee was read by the Rev. Professor Peacock.

ADDRESS

BY

THE REV. W. VERNON HARCOURT.

A few weeks since I bade farewell to one whose friendship I owe to this Association, setting forth on an enterprize full of labour and hazard, but full also of such visions of glory, and so brilliant a prospect of scientific conquests, that for a mind combining the high aims of the philosopher with the intrepidity of the sailor, no danger, no difficulty, no inconvenience seemed to exist, even in those regions where

> Stern famine guards the solitary coast,
> And winter barricades the realms of frost.

We sat down, Gentlemen, before his chart of the Southern Seas, and the unapproached pole of the earth: he showed me his intended track; he pointed out the happy coincidence of the recent discovery (one of the debts which science owes to commerce) of two small islands in those seas; he put his finger upon the spot which theory assigns for the magnetic pole of verticity, corresponding to that which he had himself discovered in the opposite hemisphere, and so situated, intermediately between the two newly-discovered insular stations, that should he not reach the pole itself, they would enable him to verify or correct the theory; and again, on the spot where his course would cross the point of maximum intensity which the same theory involves. We next reviewed the places at which he is commissioned to plant, on his way, three magnetical and meteorological observatories,—St. Helena, the Cape, and Van Diemen's Land, and those at which he himself especially wished to observe,—at Kerguelen's Land, New Zealand, and other stations on the land and ice; and we talked of all these as part only of a system of observations simultaneous or combined, stretching from one side of the earth to the other, undertaken or promised, through the whole extent of the British empire, from Montreal to Madras, and blending in co-operation with chains of observatories established, or on the point of being established, by other nations in the four quarters of the world.

I confess, Gentlemen, I felt, as one of the white and bright moments

of life, such a conversation, at such a moment, with a man, of whom, as he is no longer with us, I may venture to say, that he is worthy of being employed on so glorious a service.

When I had bidden him adieu, I had leisure to reflect on the possible consequences of his expedition, and the plan of which it forms a part,—The problem of terrestrial magnetism solved—first, the laws of the changes of its elements detected, their constant parts determined, and the whole proved to coincide with a theory based on *a legitimate representation of known facts*—then, the lines of its force and direction truly drawn, the deviations predicted, and the corrections supplied—in the immediate view of practical consequences, our ships finding in their compass-needles a more unfailing guide than in the fragile timepiece or the cloudy sky—in the distant horizon of higher and yet more fruitful speculation, the *true cause* of the phænomena—and therein perhaps a completion of what Newton began—a revelation of new cosmical laws—a discovery of the nature and connexion of imponderable forces—all these the possible results of approaching the heights of theory on what may prove to be their most accessible and measurable side.

Afterwards I thought of the causes which had conduced to this grand undertaking, which had prompted the British Government to seek these laurels, and cull these fruits of peace, by the outfit of the most important and the best-appointed scientific expedition which ever sailed from the ports of England. Well were the government both prompted and seconded by the science of the country. I saw the apartments of the Royal Society moved by a fresh spirit of energy and zeal; its most distinguished members sacrificing personal considerations, and postponing individual to public objects—Committees meeting and corresponding, to perfect the instruments of observation, and prepare the plans for observing—distant members, at Dublin and Woolwich, deputed to instruct the observers. It seemed as if the days of Wallis, and Wilkins, and Wren, and Boyle, and Evelyn were revived; and whom did I recognise, Gentlemen, among those who were thus zealously and effectively employed? Their faces were familiar to me; they were the same men who first proposed the subject, and discussed it together at the meetings of this Association, the same who went from you to call the national attention to it, and had since added to that call all the influence and all the efficacy of the Royal Society. The individual, again, appointed to command the expedition, and conduct the observations, and those also who were selected to instruct the other observers, who were they? They were not only taken from the ranks of the Association, but they had perfected the instruments of observation,

and gained additional experience in observing, during their co-operative labours in its service. When Captain James Ross and Major Sabine, and Profs. Lloyd and Phillips, and Mr. Fox, were engaged in ascertaining the curves of the magnetic elements across the British islands, with unexampled completeness, they performed a national work important in itself, but still more important as leading on to greater undertakings. But lastly, Gentlemen, whence proceeded that theory which it is the highest object of all this philosophical energy, and all this national liberality to put to the test—the first profound attempt to bring the magnetism of the earth under the dominion of calculation? Let the illustrious author of it speak for himself: "Several years ago," says Gauss, "I repeatedly began attempts of this kind, from all of which the great inadequacy of the data at my command forced me to desist." "The appearance of Sabine's map of the total intensity, in the 7th Report of the British Association for the Advancement of Science, has stimulated me to undertake and complete a new attempt,"—an attempt, Gentlemen, which, whether we consider the importance of its results, or the labour and the strength expended upon it by this great mathematician, reflects high credit on the author of the map, which provided for such a theory numerical expressions, and does honour to the Institution which was in any the least degree instrumental to its production.

In what I have been saying, Gentlemen, I have been desirous of pointing to that spirit of co-operation which our meetings have called forth in this country. We see among us at length the novelty of many fraternities of fellow-labourers working for a common cause, on a common plan, with a perfect mutual understanding. This is the only means of advancing the great branches of knowledge in which space is a necessary element, and it is the best security for a constant progress in all. Science, in a country where every man labours alone, has periods of darkness as well as light, and resembles those stars which are seen from time to time to "pale their ineffectual fires"; but there need be no fear of its decline, there can be no check in its advance, when it depends not on the prowess of any single arm, but on the force of its numbers and the order of its array.

The system of your meetings, Gentlemen, has brought together things which ought never to be disjoined—the principles of science, with their application to human use. After gathering your first members from our ancient schools of learning, you passed to the marts of commerce, and are now come to the heart of the manufactures of England, and look round on all the resources and creations of mechanical

art. The theorist and mechanician here meet together to the mutual advantage of both; witness on the one part the instrument now working in the Philosophical Institution of this town*, and almost supplying the place of a constant observer, which is about to measure the force of the wind at every instant of time, at St. Helena, the Cape of Good Hope, Van Diemen's Land, and near the southern Pole. On the other hand, I may mention an anecdote which shows by how circuitous a route art has sometimes been driven to seek the aid of science. During the war between France and England, a Frenchman brought with him the discovery of a great chemical philosopher in Paris, to barter for a secret of the English manufactures; not finding in Lancashire the person he sought, he left a message, returned to London, and was imprisoned under the Alien Act; to prison, however, the English manufacturer followed him, obtained his secret and his liberation, made his own fortune, and enriched his country†.

But need I go further than the immediate vicinity of this town for an instance, the most striking on record, of the mighty influence which the introduction of a new principle in science can exercise on all the arts of life? The history of the improvement of the steam-engine by Watt finely illustrates this truth. In the eulogium of that great man lately published, the Secretary of the French Academy has justly and eloquently displayed, by this memorable example, the power which resides in the unaided genius, industry, and patience, of a single individual, applying his mind to the fruitful application of a scientific truth, and the incalculable extent to which he may promote the welfare of his country, and benefit the whole family of mankind. He has taught us also to reflect "in what an humble condition of life those projects were elaborated which were destined to carry the British nation to a degree of power hitherto unheard of‡."

But whilst I refer you to this volume, Gentlemen, for an admirable exposition of important truths, I feel myself called upon to state, that the zeal of M. Arago has carried him too far, when it has tempted him to transfer to Watt those laurels which both time and truth have fixed upon the brow of Cavendish.

It is far from my views, to draw any comparison between two illustrious names, of which one stands as high in the discovery of natural facts, as the other does in their useful application; but let

* Mr. Osler's self-registering Anemometer.

† This anecdote, with the names of the individuals, was related to me by the late Dr. Henry.

‡ *Annuaire, pour l'an* 1839. p. 236.

us hold a just and even balance between genius that rises superior to the pressure of circumstances, and that which reaches to at least equal intellectual heights, unseduced by rank and riches. The Secretary of the Academy has not confined himself to taking from Cavendish the honour of his discoveries, but has cast a cloud of suspicion on his veracity and good faith: he has, in fact, imputed to him, the claiming discoveries and conclusions which he borrowed from others, of inducing the Secretary of the Royal Society to aid in the fraud, and even causing the very printers of the Transactions to antedate the presentation-copies of his paper.

Yet this, Gentlemen, is the man to whom, at his death in 1810, one who knew and was competent to speak of him bore the following testimony:—"Of all the philosophers of the present age," said Davy, "Mr. Cavendish combined the greatest depth of mathematical knowledge with delicacy and precision in the methods of experimental research. It might be said of him, what, perhaps, can hardly be said of any other, that whatever he has done has been perfect at the moment of its production: his processes were all of a finished nature; executed by the hand of a master, they required no correction; and, though many of them were performed in the very infancy of chemical knowledge, yet their accuracy and their beauty have remained unimpaired, amidst the progress of discovery, and their merits have been illustrated by discussion, and exalted by time. In general, the most common motives which induce men to study, is the love of distinction and glory, or the desire of power; and we have no right to object to these motives; but it ought to be mentioned, in estimating the character of Mr. Cavendish, that his grand stimulus to exertion was evidently the love of truth and knowledge: unambitious, unassuming, it was often with difficulty that he was persuaded to bring forward his important discoveries. He disliked notoriety; he was, as it were, fearful of the voice of Fame; his labours are recorded with the greatest simplicity, and in the fewest possible words, without parade or apology; and it seemed as if, in publication, he was performing, not what was a duty to himself, but a duty to the public."—"Since the death of Newton," he concludes, "England has sustained no scientific loss so great as that of Cavendish: his name will be an object of more veneration in future ages, than at the present moment; though it was unknown in the busy scenes of life, or in the popular discussions of the day, it will remain illustrious in the annals of science, which are as unperishable as that nature to which they belong; it will be an immortal honour to his house, to his age, and to his country."

Alas, Gentlemen, for human predictions and posthumous fame! Who could have foreseen that, ere thirty years had elapsed, so opposite a view of the labours and character of this philosopher would proceed from one of the most enlightened of his successors? But for the sake of justice, and because there is no page in the history of experimental philosophy more instructive than that to which this question carries us back, I now ask permission to give equal publicity to a different view, and to offer such a sketch of the great chemical discovery of the composition of water, as may perhaps help to elucidate the truth.

According to the statements of this publication, the person who brought the first evidence of the composition of water, by proving that the water produced is equal in weight to the gases consumed in its production, was Dr. Priestley; the person who first drew the conclusion that water is composed of oxygen and hydrogen, was Watt. Now, the former of these statements has not only no real foundation, but is contradicted by the repeated assertions of Priestley himself, who constantly maintained, that in no experiment made with care had he ever found the weight of the fluid produced, equal to the sum of gases, or the fluid itself pure water. The latter, Gentlemen, has no foundation, except in the licence which M. Arago has used, of quoting the words of Watt otherwise than they really stand. Nor can there be a stronger instance of the inconvenience of such translations, than the difference of meaning and value in the words thus substituted for each other—*hydrogen*, for example, put for *phlogiston*.

What is it, Gentlemen, that gives importance to this discovery in the history of science? Not merely, as has been too popularly stated, that it banished water from among the elements, but that whilst it accounted for an infinite number of phænomena, it introduced into chemistry distinctness of thought and accuracy of reasoning, and led to the general prevalence of a sounder logic. The prejudice of that epoch was, not to regard compound substances as simple, but to consider undecompounded substances as compound. The hypothesis, that a principle called Phlogiston entered into the composition of a great variety of bodies which we now consider simple, had infected the whole of chemistry. This hypothesis, at first but a conjectural attempt to generalise the phænomena of combustion, gradually made itself a coat of patchwork out of the successive discoveries of half a century, and arrived at playing as many feats in philosophy, as the harlequin in a pantomime. In the very paper of Watt on which this claim is founded, we find, first, inflammable gas, then charcoal, then sulphur, then nitrogen, to be

all different forms of the same phlogiston, united with a minute portion of different bases; we find it combining with oxygen, in one proportion, to form carbonic acid, in another, nitrogen, in another, water. Its affinities with these bases, and with all the metals, had been determined by Bergman, as well as the relative weights in which it entered into composition: and to complete all, the year before the publication of Cavendish's experiments, Kirwan had proceeded to give a table of the *absolute* weights—had computed for instance that fourteen cubic inches of nitrous air contain 0·938 of a grain of phlogiston, and had actually deduced a law for these weights, corresponding with the specific gravities of the metals.

You will easily conceive, Gentlemen, the effect on a purely experimental science of such a hypothesis as this, and you must add the effect of other hypotheses, equally prevalent, which bestowed similar chemical affinities on the principles of light and heat. Bergman calculated the weight of phlogiston "*in pollice cubico decimali*" of hydrogen to be $\frac{9}{100}$ of a pound, and the weight of specific heat in the same to be $\frac{6}{100}$ of a pound. His method of arriving at results which have such a face of precision furnishes a very curious specimen of analytical reasoning. He *assumes*—1st, That *charcoal* consists of fixed air, alkaline earth, and *phlogiston:* he ascertains as well as he can the weight of the two former constituents, and calculates that of the latter from *the loss* in his analysis. 2nd, He *assumes* that *phlogiston* exists in *iron* in the ratio to that in charcoal of their respective effects in *phlogisticating*, or *alkalizing*, an equal quantity of nitre; he determines this proportion, and from the 1st experiment deduces the absolute weight of phlogiston in a given weight of iron. 3rd, He *assumes hydrogen* to consist of *phlogiston*, and *matter of heat;* he *assumes* further that the phlogiston in a given volume of hydrogen is proportionate to the phlogiston in the iron from which it is evolved by the action of acids; he determines by experiment what the weight of iron is which produces a given volume of hydrogen, and he concludes from the two data before obtained *the absolute weight of the phlogiston*; this he subtracts from the total weight of the hydrogen, and thus determines *the absolute weight of the matter of heat**. In like manner you find the ideas of Watt respecting the composition of water connected with, and springing out of the idea, that it was reconvertible, *not simply into phlogiston and dephlogisticated air, but, by an intimate union of the latter with the principle of heat, into phlogiston and atmospheric air.* By such loose

* See Bergman 'de Attract. electivis', pp. 413, 440, 'de Analysi Ferri,' p. 24, *Opuscula Phys. et Chem.* vol. iii. Upsal. 1783.

reasoning as this, some of the best chemists of the day were misled, not only as to the direction of their labours, but even the results of their experiments. But in Cavendish's celebrated inquiry into the causes whereby air suffers diminution in a variety of processes then termed phlogistic, it is well worthy of remark how steadily he moves on from truth to truth, on every point on which experiments afforded ground for reasoning, unfettered by the complexity of the phlogistic theory; and it is equally remarkable, how loose he sits to the favourite hypothesis to which the rest of his countrymen clung with such persevering tenacity. He, first of all his contemporaries, did justice to the rival theory recently proposed by Lavoisier, and weighed it in equal scales before the public eye. He alone seemed to understand, as it became a disciple of the school of Newton, the true use of a hypothesis: he valued neither system otherwise than as an expression of facts, or as a guide to future inquiry. He took these opposite hypotheses, and retrenched their superfluities; he pared off from both, their theories of combustion, and their affinities of imponderable for ponderable matter, as complicating chemical with physical considerations; and he then corrected and adjusted them with admirable skill to the actual phænomena, not bending the facts to the theory, but adapting the theory to the facts.

Allow me to give you an instance of this adaptation. Priestley had stated, that he had converted charcoal into inflammable gas by the simple action of the burning lens, and obtained it from pure iron, by the same means, and had drawn the consequence, that iron was composed of phlogiston united to the basis, or *calx*, of iron, and that charcoal and inflammable gas were pure phlogiston. "I had no suspicion," he says*, "that water was any part of inflammable air;" "yet that water in great quantities is sometimes produced from burning inflammable and dephlogisticated air, seemed to be evident from the experiments of Mr. Cavendish and M. Lavoisier. I have also frequently collected considerable quantities of water in this way, though never quite so much as the two kinds of air decomposed." "Afterwards, seeing much water produced in some experiments in which inflammable air was decomposed, I was particularly led to reflect on the relation which they bore to each other, and especially *Mr. Cavendish's ideas on the subject.* He had told me, notwithstanding my former experiments, from which I had concluded that inflammable air was pure phlogiston, he was persuaded that water was essential to the production of it, and even entered into it as a constituent principle. At that time I did not perceive the force

* Priestley on Air, ed. 1790, part 3. sect. 4.

of the arguments which he stated to me, especially as, in the experiments with charcoal, I totally dispersed any quantity of it with a burning lens, *in vacuo*, and thereby filled my receiver with nothing but inflammable air. I had no suspicion that the wet leather on which my receiver stood could have influence in the case, while the piece of charcoal was subject to the intense heat of the lens, and placed several inches above the leather. I had also procured inflammable air from charcoal in a glazed earthen retort two whole days successively, in which it had given inflammable air without intermission: also iron filings in a gun-barrel, and a gun-barrel itself had always given inflammable air whenever I tried the experiment." "But, my attention being now fully awake to the subject, I found that the circumstances above mentioned had actually misled me." "Being thus apprised of the influence of unperceived moisture in the production of inflammable air, and willing to ascertain it to my perfect satisfaction, I began with filling a gun-barrel with iron filings in their common state, without taking any precaution to dry them, and found that they gave air as they had been used to do;" "at length however, the production of inflammable air from the gun-barrel ceased, but on putting water into it, the air was produced again; and a few repetitions of the experiment fully satisfied me that I had been too precipitate in concluding that inflammable air is pure phlogiston." Dr. Priestley afterwards gives an account (Phil. Trans. 1785) of his repeating Lavoisier's celebrated experiment, in which the decomposition of water was proved by passing steam through an iron tube. "I was determined," he says, "to repeat the process with all the attention I could give to it; but I should not have done this with so much advantage if I had not had the assistance of Mr. Watt, who always thought that M. Lavoisier's experiments by no means favoured the conclusion that he drew from them. As to myself, I was for a long time of opinion that his (Lavoisier's) conclusion was just, and that the inflammable air was really furnished by the water being decomposed in the process; but though I continued to be of this opinion for some time, the frequent repetition of the experiments, with the light which Mr. Watt's observations threw upon them, satisfied me at length, that the inflammable air came from the charcoal or the iron."

It appears from these statements, and may be still more clearly gathered from Cavendish's own remarks * in his "Experiments on Air," that he not only set Priestley right as to his supposed fact of the production

* Phil. Trans. vol. lxxiv. p. 137.

of hydrogen from *dry* iron, but furnished a theory by which the disciples of phlogiston* might nevertheless maintain their ground both in this and other cases. The explanation which his theory afforded in this instance was, that the inflammable air, due to the unperceived moisture in the iron filings, or in the air of the vessels, is evolved by the force of double affinities in the following manner—the water, decomposing the iron, combines in part with its basis, and in part with the phlogiston (or dry hydrogen) which it was supposed by hypothesis to contain; forming by the one combination the calx, or oxide, of iron, and by the other, inflammable gas†. Such a representation was not incompatible with any known facts, and Cavendish had his own reasons for giving it on the whole a preference over that which seems to us so much more plain and reasonable: its fault as a theory was, that it was needlessly hypothetical, and that it was part of a system overloaded with a multitude of hypotheses.

Lavoisier was the first to introduce into chemistry a juster language and a safer manner of stating facts; he caught sight of a principle which has been since laid down by Davy as a general proposition, and has contributed much to the distinctness of chemical science,—the principle that every body is to be reasoned about as simple till it has been proved by direct evidence to be compound. To Cavendish, trained in the rules of demonstration, and gifted with a sagacity and clearness of conception beyond his fellows, hypothetical thoughts and expressions were no stumbling-block; and he seems therefore not to have felt how great an obstacle they present to the general movement of science as it floats upon the tide of a thousand understandings.

If the question then be, who reformed the expressions and logic of chemistry, or who furnished the simple terms in which we now state the elements of water? the answer is, Lavoisier; but if it be, who discovered and unfolded the most important facts on which that reforma-

* That Watt derived from Cavendish his views on this subject, is evident from the parenthetical introduction of his altered opinion that inflammable gas was not pure phlogiston, but a combination of phlogiston and water, in the middle of experiments and arguments to prove the contrary, without assigning any reason, and after the publication of Cavendish's theory. See *Mr. Watt's Thoughts*. Phil. Trans. vol. lxxiv. p. 330.

† Cavendish assigns as his principal reason for believing inflammable gas to be *a compound* of this description, that *it does not unite with oxygen at common temperatures*; but it is likely that he was influenced also by the result of his experiments on "a different kind of inflammable air, namely, that from charcoal," for which, see Postscript, p. 38.

tion relied? who detected and proved the composition of water, and deduced the train of corollaries which flowed from it? the answer is, Cavendish. The discovery was not one of those which was within every man's reach, especially in an age of loose experiment and inconclusive reasoning: it was one which could never have been made, but by a strict appreciation of quantities, and a careful elimination of the sources of error: it formed part of the solution of the difficult problem, which at that time occupied the attention of all the chemists in Europe—the cause of the diminution of atmospheric air, in six several cases—in the passage through it of the electric spark, in its burning with hydrogen, in its contact with nitrous gas, in respiration, in the inflammation of phosphorus and sulphur, and in the calcination of metals. These phænomena had been accounted for by a supposed phlogistication of the air, and a consequent formation and absorption of carbonic acid; Lavoisier stood alone in attributing the phænomena of the four last classes to their true cause. When Cavendish took up the problem, he began by proving that no carbonic acid was necessarily produced in any of these processes: and then, Gentlemen, he turned to use a well-timed but incorrect experiment of an inhabitant of this town, (Mr. Warltire,) and he made it the basis of a series of analytic and synthetic researches, unequalled then, and never since surpassed, by which he demonstrated, as a means of arriving at the solution of his problem,—1st, The quantitive composition of the atmosphere; 2nd, The combination of hydrogen with oxygen and the quantitive composition of water; 3rd, The chemical union of nitrogen with oxygen and the constitution of nitric acid. From these experimental data he deduced the true causes of the diminution of the air in the burning of hydrogen, and in the passage of the electric spark; he adopted Lavoisier's conclusion respecting the burning of metals and other inflammable matters, gave the true account of the composition of the nitrates of potash and mercury, explained the constitution of vegetable substances, and the origin of the oxygen which they exhale, and finally corrected the premature generalization which had led the French philosopher to consider that gas as the principle of acidity.

Such, Gentlemen, were the splendid results of this investigation, such the reinforcement which Cavendish brought to the nascent reformation of chemistry. Equally worthy of observation were the means employed to obtain them. The experiment to be made was the combustion of hydrogen with common air; or, as it proved, its combination with the proportion of oxygen which the common air contained. Now this latter was then a quantity imperfectly known. Hence those analyses

which he made in 1781, of the atmosphere under all circumstances, at different times of the day, in town and country, in summer and winter, by which he determined its composition more accurately than any of his contemporaries, and with a precision which has scarcely since been exceeded: thus, with a knowledge of the specific gravities of the gases, and of the weight of common air, he was in a condition to have compared the correspondence of the weight of the gases consumed in the combustion, with that of the fluid produced. But this experiment had a weak side, in the practical difficulty of collecting the fluid: he therefore took a more certain method of examining the question by volume instead of weight, by ascertaining whether the production of the fluid was accompanied by the total disappearance of the combining gases: to a given bulk of atmospheric air, he added, in successive experiments, a gradually decreasing volume of hydrogen gas, and found a point at which the computed volume of oxygen entirely disappeared. But there was yet a possibility of error: the fluid produced might contain something besides water: he analysed it, and found that the water was pure. Not yet satisfied, he repeated the experiment in a simpler form, by burning the hydrogen with oxygen, in place of common air; and here a difference little to have been expected appeared, for, on analysing the fluid he found it to contain not water only, but nitric acid; he traced the acid to its source in the small portion of atmospheric air with which the gases chanced to be contaminated, and inferred that the oxygen and nitrogen which it contains, unite under certain circumstances to form nitric acid. Thus he was led to the discovery of the cause of its diminution when traversed by the electric spark, and from the residuary defect of the experiment he completed the solution of the problem.

The experiment by which Cavendish had in 1781 ascertained the conversion of oxygen and hydrogen into water, Priestley *repeated* in an imperfect manner in 1783; and since it is *this repetition* which M. Arago has mistaken for the first proof of the composition of water, listen, Gentlemen, to Priestley's own preface to the account he gives of it: "Still hearing," he says, "of many objections to the conversion of water into air, I now gave particular attention to *an experiment of Mr. Cavendish's concerning the re-conversion of air into water, by decomposing it in conjunction with inflammable air*." He then relates the precautions he took in repeating this experiment, expresses his wish that he had a nicer balance, and tells how he collected the fluid by wiping the inside of the glass with filtering-paper; but it does not appear, either from his own statement or the still more particular

one furnished by Mr. Watt, that he examined the nature of the fluid. Experiments thus conducted could not, and did not, lead to any solid conclusion; Watt suspended his judgment upon them for a twelvemonth, and seven years afterwards, we find Priestley expressing himself thus: "*I must say, as I did when I was myself a believer in the decomposition of water, that I have never been able to find the full weight of the air in the water produced by the decomposition.*" And again: "*Having never failed, when the experiments were conducted with due attention, to procure some acid whenever I decomposed dephlogisticated and inflammable air in close vessels, I concluded that an acid was the necessary result of the union of these two kinds of air, and not water only**." Compare these statements, Gentlemen, which have stood on public record for half a century, with those of M. Arago, affirming that Priestley was the first who proved, and Watt the first who understood, *the conversion of air into water*, and ask yourselves, how it is possible in the face of such evidence to sustain a charge against Cavendish, Blagden, and the printers of the Royal Society's Transactions, of conspiring to steal a discovery *thus acknowledged to have been derived from Cavendish, and of which the truth, recognised for a moment, was immediately afterwards denied by Priestley, and doubted of by Watt.*

In doing this justice to an injured name, I have been led to speak of one whose numerous discoveries attracted in those days the eyes of all Europe to Birmingham, and who deserves to be admired not more for his inventive fertility and indefatigable industry in experiment, than for the honest candour with which he related every fortuitous success and extraneous hint, and the liberal profusion with which he scattered his gold abroad for public use, as fast as he drew it from the mine. It has been one of the charges, Gentlemen, against this Association, that an analysis of the character of Priestley formed a part of its early transactions: that character, drawn by a hand no less judicious than skilful*, regarded science alone, and contained not a single particle of political or polemical alloy: if it had, being in the chair when it was read, I should have felt it to be my duty to interfere. Much more would I myself avoid the touching from this chair on any topic which should have a tendency to excite feelings alien to *our* pursuits, and destructive to *all* social union: but whilst I can well bear to hear our meetings upbraided with such faults as these, there is one point of attack on which I think I ought not to be silent, even though it stands close on the boundaries of those subjects which I would most rigidly exclude.

* The late Dr. Henry.

On my own judgement alone I should scarcely venture to meddle with so arduous a question, did I not see those around me who desire it at my hands, as required by the position in which the Association stands.

A century and a half ago the Royal Society met with opponents similar to those whom the Association has to encounter now. "Their enemies," says Dr. Samuel Johnson, "were for some time very numerous and very acrimonious, for what reason it is hard to conceive, since the philosophers professed not to advance doctrines, but to produce facts, and the most zealous enemy of innovation must admit the gradual progress of experience, however he may oppose hypothetical temerity." They were assailed, Gentlemen, with jokes as well as libels; but there is reason even in ridicule; and, on this subject, the irony of Butler himself is forgotten; but there was also a graver class of men in those days, who saw in the establishment of the Royal Society injury to religion; their names and publications have perished, but the memorial of their apprehensions is embalmed by a writer* whose early history of the Society has been described by his great biographer as "one of the few books which selection of sentiment and elegance of diction have been able to preserve, though written upon a subject flux and transitory."—"I will now proceed," said the episcopal historian, "to the weightiest and most solemn part of my whole undertaking,—to make a defence of the Royal Society and this new experimental learning, in respect of the Christian faith; and I am not ignorant in what a slippery place I now stand, and what a tender matter I am entered upon; I know it is almost impossible, without offence, to speak of things of this nature, in which all mankind, each country, and now almost every family, disagree. I cannot expect that what I shall say will escape misrepresentation, though it be said with the greatest simplicity, while I behold that most men do rather value themselves and others on the little differences of religion than on the main substance itself." He then thinks it necessary to employ thirty-three pages in defending the inductive philosophy against the charge of impiety, and concludes with this caution,—"*that, above all, men do not strive to make their own opinions adored, while they only seem zealous for the honour of God.*"

These are bygone days, and Time, Gentlemen, which seems to have little effect in removing prejudice, makes great changes at least in circumstances: the philosophy thus early dreaded has since extended itself on every side; science pervades our manufactures, and science is penetrating to our agriculture; the very amusements, as well as the

* Spratt, Bishop of Rochester.

conveniences, of life, have taken a scientific colour. In these altered circumstances, were any now rash enough to kindle the dying embers of this obsolete bigotry—to stir up a worse than civil war between the feelings of piety and the deductions of reason, to go forth with the "*argumentum ad odium*" for their only weapon, against a host of facts patiently ascertained, and inferences fairly drawn;—were they to call in the Scriptures to supply their defects, and fasten on *them* their own crude and ignorant speculations—were they to be seen shifting their ground from one false position to another, all equally untenable, and all assuming to be the sole defences of the true faith,—what would be the natural consequence of a warfare at once so offensive and so hopeless? what the effect of so many baffled aggressions and self-inflicted defeats? what the fruit which the tree of knowledge would bear, thus injured, in the name of religion, by men who should remove the boundary marks of faith and philosophy, and confound things human and divine?

There are, indeed, certain common points in which reason and revelation mutually illustrate each other; but in order that they may ever be capable of doing so, let us keep their *paths* distinct, and observe their *accordances* alone; otherwise our reasonings will run round in a circle, while we endeavour to accommodate physical truth to Scripture, and Scripture to physical truth.

The observation of the true points of accordance in such lines, is one of the most instructive of all studies; and when combined with an honest observation of the discordances also, leads to important conclusions.

There are many branches of inductive inquiry through which these parallel lines may be drawn, and their accordances observed. Thus Sir Isaac Newton has deduced from the history of inventions, the spread of nations, and the present amount of population, that the time for which mankind have existed cannot materially differ from that usually assigned from Scripture. Geology, with less distinctness, points towards the same conclusion. But there are lines of accordance and discordance *within the Scriptures themselves*. Now, in drawing these lines for human chronology, we find discordances between different versions of almost equal authority, and *that* to such an amount, that while the Hebrew gives 1948 years for the epoch from the Creation to Abraham, the Greek assigns for the same period 3334; and this difference of nearly 1400 years lies not in a single sum, but is divided among successive generations. For the first seven centuries, the larger computation was exclusively followed; for the last four, the whole Western

Church has adopted the less. Do these discordances undermine the authority of Scripture? Do they shake—has any one imagined them to shake—the substantial credit of its history of mankind? Yet it is plain that, in its present condition, Scripture does not teach with certainty and exactness the computation of time. The book of Genesis is not then a book of chronology: it is a book which, by a series of genealogies, traces back the various races of men to one common source. Now, in this point, all the versions, and both the volumes of Scripture, concur—to this point all the lines of scientific inquiry converge—the analysis of language, the most legitimate conclusions of physiology and natural history, coincide in the fact *that the nations of the earth are of one blood.* There is nothing vague or doubtful in this. Reason and religion are here in perfect accord.

Let us proceed from the history of mankind to the general philosophy of nature. No one, I think, can doubt that those who condemned the Copernican system were justified in conceiving that *the Scriptures speak of the earth as fixed, and the sun as the moving body.* Every one will allow also that this language is ill adapted to the scientific truths of astronomy. We see the folly of any attempt, on this point, to interpret the laws of nature by the expressions of Scripture: and what is the ground of our judgement? We are not all competent to judge between the theory of Copernicus and those which preceded it; but we determine against the seeming evidence of our senses, and against the letter of Scripture, because we know that competent persons have examined and decided the physical question. Now, Gentlemen, *in Geology we are arrived at the selfsame point;* that is to say, a vast body of the best-informed naturalists have examined, by all the various lights of science, and by undeniable methods of investigation, the structure of the earth; and however they may differ on less certain points, they all agree in this—that the earth exhibits a succession of stratification, and a series of imbedded fossils, which cannot be supposed to have been so stratified, and so imbedded, in six days, in a year, or in two thousand years, without supposing also such numerous, such confused, and promiscuous violations of the laws and analogies of the universe, as would confound, not the science of geology alone, but all the principles of natural theology. Here, then, is another point of discordance: *and in both these cases the discordance lies between the language of Scripture and the truths of science.*

To understand how this may be explained, let us compare the account of creation given in Genesis with that contained in a composition as old, or older, than this oldest of books,—a composition which, car-

rying us back some four thousand years into the midst of the patriarchal ages, yet breathes a spirit of no vulgar philosophy; and when it speaks of Him " who hangeth the earth upon nothing," who " maketh a weight for the winds, and weigheth the waters by measure," might tempt us to seek here, with Hutchinson, for the true system of the universe. In that book, I say, we have the first account of the creation of the world, proceeding, as it were, from the mouth of the Creator himself. " The Lord answered Job out of the whirlwind, and said, Where wast thou when I laid the foundations of the earth? declare if thou hast understanding. Who hath laid the measures thereof, or who hath stretched the line upon it, whereupon are the foundations fastened, or who laid the corner-stone, when the morning stars sang together, and all the sons of God shouted for joy?" " Or who shut up the sea with doors, when it brake forth as if it had issued from the womb, when I made the cloud the garment thereof, and thick darkness its swaddling band, and brake up for it my decreed place, and set bars and doors, and said hitherto shalt thou come, and no further, and here shall thy proud waves be staid?"

Take, then, these " thoughts that breathe and words that burn," and compress them if you can into some true or some fanciful system of science; teach us where to find " the house wherein darkness dwelleth," to " bind the sweet influence of the Pleiades, and loose the bands of Orion"; explain to us, with respect to one of God's creatures, what the natural process is by which he " drinketh up a river and hasteneth not"; and of another, how " his breath kindleth coals, and a flame goeth out of his mouth," and then take credit to yourself for vindicating the truth of Scripture: and when you have thus illustrated a composition, by the side of which, till you touched it, the images of Homer and Pindar seem but as prose, go on—instruct us how to interpret that other most ancient book, recorded, it has been thought, by the very same hand—take that passage of it which drew forth the admiration of heathen antiquity—borrow for your purpose the deepest thoughts of modern science—substitute, " Let there be *ether*, and there was *ether*," for " Let there be light, and there was light." Why does this altered expression fall so flat upon the ear? it is not like the flood of harmonious sound which some of you may have heard from this Orchestra responding to the words—it is not like the words themselves, which pour upon the mind at once all the beautiful irradiation and delightful perceptions of light: and yet, Gentlemen, after all, you have not even thus perhaps presented a pure scientific view of the act of creation; for when you have conceived this empyreal ether, this boundless and

all-pervading substance, which vibrates knowledge to our wondering ken out of the unfathomable depths of space, are you prepared to take here your stand? may you not yet find other and still finer links beyond? and if you should, must not the very form of expression, the instantaneous connexion of the thing made with its Maker,—"He spake, and it was created"—become as little scientific, as it is, to the understanding of all men, superlatively impressive and sublime?

You see, Gentlemen, what my meaning is. Had it pleased God to endow the ministers of religious truth with supernatural knowledge of the mysteries of nature, they could not have used that knowledge to any practical purpose; they could not have used it so as to carry the truths they were commissioned to preserve, into the hearts and imaginations of mankind; and so entirely sensible were they themselves of this, that on subjects thus passing the power of language, they not only meddled not with any system of science, but passed over popular ideas, and the common senses of words, to those highly figurative expressions, which are best adapted to impress transcendental truth.

Who, then, would expect to find in Genesis the chronology or sequence of Creation? who can think that he upholds the authority of Scripture by literal constructions of *such a history*, by concluding from them that the earth was clothed with trees and flowers before the sun was created, or that the great work was measured by six rotations of the earth upon her axis? It scarcely needed the evidence of physical or geological science to teach us that such a mode of interpreting the sacred writings is utterly unsound: when the same author speaks of man as created *in the image of God*, every one perceives that this is one of the boldest figures which language can produce; and in what but a figurative light can we view *the days of Creation?* what can we find in such a description but this truth—*that the six grand classes of natural phænomena were, all and each, distinct acts of Divine power, and proceeded from the fiat of a single Creator?*

Here, Gentlemen, is a second instance of one of those great points of accordance, where all the conclusions of human science coincide with revealed religion, and none more remarkably than that which has been so falsely termed *irreligious* Geology; for as Astronomy shows the unity of the Creator through the immensity of space, so does Geology, along the track of unnumbered ages, and through the successive births of beings, still finding in all the uniform design of the same Almighty power, and the varied fruits of the same unexhausted goodness.

Thus, Gentlemen, we have seen in this comparison of two collateral

lines of knowledge, certain points of accordance, and certain points of disagreement; we have seen that Scripture furnishes us with no perfect chronology of history, and with no chronology of creation except the creation of man; but we find, also, that it does provide for us, and has evidently aimed at providing for us, from the earliest times to the present hour, the knowledge of two facts: *that all men are the children of one human father*, and *the handiwork of one Almighty God.* Here the coincidence is perfect of every line within and without; here the philosophy of Job and Moses, of every prophet and evangelist, agree; here all the inductions of every branch of science mark the same corresponding points.

And what, Gentlemen, is the common quality of these two facts? Are they not the very facts on which the system of human duty subsists, on which humanity and piety depend?

These truths, nursed for a thousand years in the ancient Scriptures of the Jews, led forth into new day, and with new accessions of the same kind of knowledge by our holy religion, have walked through the world, and been believed alike by the ignorant and the wise, before our sciences were born: and here observe the methods and the course of Providence;—how, as in process of years, the current of traditionary belief runs weaker,—how, as the advance of human intellect looks for other kinds of proof, the arts and sciences come in to support these essential truths: printing gives them stability and extension; optics and astronomy pour in an infinity of evidence; comparative anatomy brings up its convictions, and geology subdues the sceptical mind with hitherto unimagined demonstrations.

And now, Gentlemen, I think we are in a condition to draw an inductive conclusion, and even to hazard a prediction. We may safely predict, that truths thus firmly established by evidence, will never be shaken by the researches of that reason which has hitherto lent them all its support; we may clearly point to that sacred ground on which no unhallowed hypothesis should tread; we are entitled, by the rules of our art, to say to the misnomered philosopher who rashly invades this territory,—These are settled points, settled by every conclusion of the intellect, as well as by every intuition of the heart: stand aloof! disgrace not the name of Science by throwing stones at the Temple of Truth. But for this assembled body of real workmen, amidst their labours of intellectual industry,—the quarrier of the stone, and the fine carvers thereof; the miner that digs the ore, and the smith that fashions it in the fire—for all who are employed on this sacred building, we are justly entitled to claim, that they shall

not be forced, like the builders of another temple, to work with arms in their hands—we are bound to wish them God speed: it is our duty, our pride, and pleasure, each in our degree, to aid their efforts, and animate their zeal. Go on, and prosper, Gentlemen, amid the best wishes of the wise and good; look well on the beauty of the fabric you are adorning, and mark its substantial utility; see piety kneeling at its altar, and human infirmities crowding to its gate. With such thoughts within, and such sympathies without, strengthen and regale yourselves amidst your toils, and remember that they carry with them a far higher reward than any human sympathy, in the approbation and the blessing of the great Father of Truth.

POSTSCRIPT.

I have taken notice in the foregoing address, that the *éloge* of Watt, delivered to the French Academy by one of its secretaries, and subjoined to the *Annuaire* for 1839 published under the authority of the Bureau des Longitudes, is blemished by statements which reflect unjustly on the character of one whose memory is cherished among us, as a bright example of the union of modesty with science, of the purest love of truth, with the highest faculties for its discovery, and the most eminent success in its attainment.

Perceiving these statements to be founded in mistake, I took the earliest opportunity of rectifying them, at the meeting of the British Association which followed within two or three weeks after I became acquainted with them, rejoicing that I had it in my power, from the position in which I had the honour of being placed, to make the correction of the error as formal and public as its promulgation had been; and persuaded that M. Arago, as soon as he should be fully possessed of the facts, would consider it a duty which he owed both to the Academy and to himself, to retract the suspicions which he had expressed.

I regret, however, to find that I have not as yet succeeded in stating the case with sufficient clearness to satisfy him, and that he continues to maintain before the Academy* the correctness of his views, corroborating them at the same time with the additional authority of M. Dumas. M. Arago says, that the account I have given of the discovery of the composition of water is incomplete (*tronqué*), and I feel it to be due to him to supply what may have been wanting in it, and to furnish him with such evidence as can no longer leave any doubt upon his mind.

* *Comptes rendus* for January 20, 1840, p. 109, No. 3.

The proofs which I have already alleged, that Cavendish owed nothing either to the experiments of which Priestley sent an account to the Royal Society, on the 21st of April 1783, or to the conclusions which Watt drew from them, were these—

1. The experiments which Cavendish made in the summer of 1781 not only necessarily involved the *notion* (which is the claim set up for Watt), but substantially established the *fact* (which is the claim set up for Priestley) of the composition of water.

2. The experiment which Priestley made in April 1783, *for the professed purpose of verifying the fact of the conversion of air into water, communicated to him by Cavendish*, added nothing to the proofs which Cavendish had already obtained of it nearly two years before.

3. Whilst the views of Cavendish are shown by the internal evidence of the experiments themselves, and the train of reasoning which they imply, to have been from the first precise and philosophical, those of Priestley and Watt were always, as regards the former, and till after the publication of Cavendish's and Lavoisier's papers, as regards the latter, vague and wavering to a degree scarcely comprehensible to those who have not studied the ideas prevalent at that period of chemical history.

These three positions I hope now to establish in a manner which will leave M. Arago nothing more to desire.

The opinion of Watt has been called a theory, a doctrine, and even a *hypothesis*; and a northern critic *, who views this question of individual justice as one of national honour, allows the claim of Cavendish to the proof of the fact, but reserves for Scotland the credit of the *hypothesis*. So far, certainly, as it involved the theories of heat and phlogiston, it *was* a hypothesis; but so far as it related to the conversion of inflammable and dephlogisticated airs into water, it was simply an opinion that Priestley had succeeded in proving the point which he, after Cavendish, had made an experiment expressly to ascertain.

Whatever it be called, however, whether a statement of the result of Priestley's experiment, or a hypothesis, or a doctrine, or a theory, it was no sooner conceived than placed by Watt on the shelf, and left there from April to November: the experiment of Priestley also remained in abeyance. Priestley has given reason enough for his not prosecuting so important an inquiry further, by informing us that it belonged to Cavendish: Watt has also assigned a reason for his suspense; and I have shown in my address, that that reason proves him

* Edinburgh Review, No. 142.

to have had no clear conception of the composition of water as consisting of oxygen and *hydrogen: it proves that with him hydrogen and phlogiston were not convertible terms.* In this I am sure MM. Arago and Dumas will agree with me, whenever they shall take the trouble to compare with attention Priestley's paper on "the Seeming Conversion of Water into Air," and Watt's reference to that paper in the preface to his letter to M. de Luc. The following is his own account of his theory. "I first thought," he says, "of this way of solving the phænomena, in endeavouring to account for *an experiment of Dr. Priestley's wherein water appeared to be converted into air*; and I communicated my sentiments in a letter addressed to him, dated April 26, 1783, with a request that he would do me the honour to lay them before the Royal Society; but before he had an opportunity of doing me that favour, he found, in the prosecution of his experiments, that *the apparent conversion of water into air by exposing it to heat in porous earthen vessels*, was not a real transmutation, but an exchange of the elastic fluid for the liquid, in some manner not yet accounted for: *therefore, as my theory was no longer applicable to the explaining these experiments, I thought proper to delay its publication, that I might examine the subject more deliberately.*" Now, what were these experiments on *the apparent conversion of water into air?* We learn from Priestley's paper, that he obtained from the distillation of water in a porous earthen retort, a constant supply of "*air of the same purity as the atmosphere*," so long as there was free access of air to the outside of the retort; and, "since," he says, "pure external air was necessary to procure good air, it was concluded by many of my friends, and especially Mr. Watt, that the operation of the earthen retort was to transmit *phlogiston* from the water contained in the [moist] clay [within the retort] to the external air*, and that the water thus dephlogisticated was capable of being converted into *respirable air* by the influence of heat." Here inflammable gas, or hydrogen, is obviously out of the question; the *phlogiston* of the water, which

* In the unpublished part of his letter, Watt states his views thus:—"On considering the last and most remarkable production of air from water imbibed by porous earthen vessels, the only case wherein it appears almost incontrovertibly that nothing was concerned in the production except water and heat, I think that the earth of the vessel attracts the phlogiston from the water and gradually conveys it from particle to particle until it transmits it to the external air, which it probably phlogisticates." "I omitted to mention in its proper place, that clay when made hot has a very powerful attraction for phlogiston, and under some circumstances, becomes quite black with it; but readily parts with it to pure air and becomes white again."

passing through the retort, is presumed to phlogisticate and vitiate the external air, is *nitrogen;* and the dephlogisticated air of the water is supposed *to retain sufficient phlogiston* to make, with the assistance of heat, *good air, of the same purity as the atmosphere.*

Such was Watt's "theory" and "way of solving the phænomena," in April 1783. But Priestley, though he did not prosecute Cavendish's experiment on the conversion of inflammable and dephlogisticated air into water, went on with his own *on the conversion of water into common air*, and discovered that instead of a decomposition of water it was only *a transmission of air through the pores of the retort:* and *therefore* it was, because the proof had failed of *the convertibility of water into dephlogisticated air, and the phlogisticated air now called nitrogen*, that Watt laid aside his theory as not borne out by the facts, till assured by the papers of Cavendish and Lavoisier, that the form in which water did contain *phlogiston*, was solely that of *inflammable gas.*

But even after the publication of their luminous views, the ideas of Watt remained equally vague; for, in his "Thoughts on the constituent Parts of Water," printed in 1784, he says, "It appears that in some circumstances dephlogisticated air can unite in certain degrees with phlogiston without being changed into water. Thus Dr. Priestley has found, that by taking clean filings of iron, which alone produce only inflammable air of the purest kind, and *mercurius calc. per se*, which gives only the purest dephlogisticated air, and exposing them to heat in the same vessel, he obtained neither dephlogisticated nor inflammable air, but, in their place, fixed air*. Phlogisticated air seems to be another composition of phlogiston and dephlogisticated air; but in what proportion they are united, or by what means, is still unknown. It appears to me, that fixed air contains a greater quantity of phlogiston than phlogisticated air does, because it has a greater specific gravity, and because it has more affinity with water." He afterwards adds, —"by some experiments of Dr. Priestley's, *charcoal*, when freed from fixed air, and other air which it imbibes from the atmosphere, is *almost wholly convertible into phlogiston*†." Does M. Arago think that one who had so confounded together hydrogen, nitrogen, and charcoal, that with him either was as likely as the other to form water, was in a condition to discover its composition, or to throw any light upon the subject? Cavendish, indeed, has shown that it was possible to reason

* Cavendish had already proved by an experiment related in the Philosophical Transactions, vol. lxxii. part 1, that Priestley was mistaken in this, and that the fixed air was owing to carburet of iron (plumbago) in the iron filings.

† Phil. Trans., vol. lxxiv. part 2, pp. 334, 351.

well on the phlogistic hypothesis, but not with ideas so loose and indefinite as these*.

I am sorry to observe that my meaning has been so far mistaken, as that I should be supposed to have imputed to M. Arago a wilful misrepresentation of the words of Watt: I know him to be incapable of any such intention. It is not bad faith that I complain of in this substitution, but want of sufficient care in stating and deciding a question of which the decision involved so severe a censure; and I do not despair of convincing both M. Arago and M. Dumas, who, after having "attentively examined the argumentation of his colleague," and having, like him, "scrupulously studied" the correspondence of Watt, preserved at Aston Hall, "adopts completely, and in all its parts, the history which M. Arago has written of the composition of water†;" that there are still more cogent proofs of the inexpedience of this substitution, and that in 1783 Watt and Priestley were almost as little acquainted with the distinctive properties of the gas which we call *hydrogen*, as they were with the word.

Though I have not had the advantage of studying the unpublished MSS. of Watt, I know *that they were submitted to the inspection of the late Dr. Henry*, with whose reputation as a pneumatic chemist M. Dumas is well acquainted, and whose knowledge, acuteness, and candour were such as eminently qualified him to judge in such a question; and I learnt from Dr. Henry that these MSS. produced no change in his opinion as to Cavendish's title to be considered the first discoverer of the composition of water. Had M. Dumas examined the account of the experiment which M. Arago quotes from Priestley's

* Priestley never extricated himself from the confusion arising out of these indistinct conceptions; in speaking of an experiment in 1785, he says, "the water must have been so far altered as to be changed into fixed air, which will be thought not to be any great paradox, if it be considered that, according to the latest discoveries, fixed air and water appear to consist of the same ingredients." Afterwards, we find him doubting whether inflammable gas and dephlogisticated air ever form water, and conceiving that they do form phlogisticated air and nitric acid (Phil. Trans., vol. lxxxi. 1791); and in his Lectures on Experimental Philosophy in 1794, he says (p. 44), "It has of late been thought that water is resolvable into dephlogisticated and inflammable air; but the experiments which have been alleged to prove this do not satisfy me; so that for any thing that appeared till very lately, water might be considered a simple element: by means of heat, however, it seems to be resolvable into *such air as that of which the atmosphere consists*, viz., dephlogisticated and phlogisticated, only with a greater proportion of the former."

† *Comptes rendus*, p. 111.

paper, with the same care which he has devoted to the correspondence of Watt, he would probably have come to the same conclusion; at all events he would not have failed to observe *one circumstance in it*, which would have rendered my statement that Priestley never found the weight of the water equal to the sum of the weight of the gases, less "*inconceivable*" than his colleague represents it: he would even have found it possible to demonstrate with some degree of certainty the minimum of the deficiency.

Priestley, though he says nothing of the weights or volumes of the gases which he burnt, any more than he does of the weight or nature of the fluid which he collected, mentions one circumstance very material to a due estimate of the value of his experiment: he tells us the means employed by him to obtain his gases pure and dry. He obtained, then, his oxygen from nitre, and his inflammable gas from *the distillation of well-burnt charcoal.* No one knows better than M. Dumas the products of such a distillation. There is reason to think that if charcoal be well burnt, and so dry that the vapour to be decomposed is small in quantity, and the heat at which it is decomposed strong, the whole product of such a distillation would be carbonic oxide and hydrogen, in equal volumes, each volume of the hydrogen evolved from the water in contact with the ignited charcoal corresponding to half a volume of oxygen, and that half-volume producing one volume of carbonic oxide. In this case, if we call the weight of the hydrogen 1·, *the weight of water formed in burning it is* 9·: the weight of hydrogen and oxygen gas burnt is also 9·; and the weight of carbonic oxide forming the other half of the distilled gas, is 14·1; and this requires for perfect combustion 8 of oxygen—together = 22·1, which added to 9 = 31·1; *so that the weight of water formed in burning such a mixture is to the weight of the gases burnt as* 9 : 31·1.

But this theoretical result has never been experimentally demonstrated*. In the experiments that have been actually made, in which no particular attention has been paid to the heat at which the decomposition is effected, part of the carbonic oxide is replaced by carbonic acid, and part of the hydrogen combines with carbon. Henry analysed this mixed gas in 1808, and found that 100 volumes, freed from the carbonic acid, required 60 of oxygen for perfect combustion, and gave 35 of carbonic acid. I shall presently have occasion to mention, that Cavendish's unpublished experiments, made in 1783, furnish nearly the

* Cruikshank's experiments, giving, as computed by Henry (Nicholson's Journal, vol. xi. p. 71), 43 measures of carb. oxide in 100 of the gas, approach nearest to the theoretical composition.

same result; and when Priestley in 1785* detected the difference between the gas distilled from charcoal, and pure hydrogen, he arrived, as to the quantity of carbonic acid, at a similar conclusion; in this case we may consider the mixture as containing 65 volumes of hydrogen, 6·66 of carburetted hydrogen, and 28·34 of carbonic oxide (in addition to 25 of carbonic acid); and it will be found, on calculating the results of burning such a composition, *that the weight of the water formed is to that of the gases burnt, as* 1 : 2.

It is manifest therefore that Priestley could not possibly, even if he had used a better method of collecting the dew, than wiping the sides of the glass, have found the weight of the water equal, as MM. Arago and Dumas suppose, to that of the gases which he burnt, nor to half that weight; it is manifest also, that he did not weigh the gases, either before burning or afterwards, nor take their specific gravity, but estimated them on the erroneous supposition that all inflammable gases are alike. Watt was in the same error; and it was in consequence of this error, that in his letter to De Luc, notwithstanding the notice Cavendish had given that "the air from charcoal is *a different kind* of inflammable air †," he states, on the authority of Priestley, that "charcoal is almost wholly convertible into phlogiston ‡." Thus do we find that Protean name bestowed *pro hac vice* on a mixture of gases probably not containing more than $\frac{1}{14}$ or $\frac{1}{15}$ of its weight of hydrogen.—"Et voilà," as Berthollet said of a subsequent experiment made with no greater accuracy, "à quoi ce réduit cette fameuse expérience de M. Priestley, dont les résultats avoient été si bien prévus et analysés par M. Cavendish §."

Cavendish, as early as 1766, or very soon after, had discovered that there was more than one species of inflammable gas: *his Phlogiston* therefore *was* hydrogen and nothing else; whether obtained from zinc or iron, he knew that it had a constant specific gravity, and he found that it had likewise a constant combining quantity; if he had not attended to these points, he could neither have experimented, or speculated, on the composition of water to any manner of purpose.

* "Expending 94 grains of perfect charcoal (by which I mean charcoal made with a very strong heat, so as to expel all fixed air from it), and 240 grains of water, I procured 840 ounce measures of air, $\frac{1}{5}$ of which was fixed air, and of the inflammable part nearly $\frac{1}{5}$ more appeared to be fixed air by decomposition." (Phil. Trans., vol. lxxv. part 1, p. 194, 1785.)

† Phil. Trans., vol. lxxiv. part 1, p. 135.

‡ *Ibid.*, part 2, p. 351.

§ Consid. sur les Exper. de M. Priestley, rélatives à la Compos. de l'Eau. Annales de Chimie, tom. iii. p. 86.

In process of time Priestley also discovered that the gas produced in the distillation of charcoal was widely different from that evolved in the solution of metals; and hence his subsequent assertion that he "had never been able to find the full weight of the air decomposed in the water produced by the decomposition," was more relevant to this question than M. Arago is aware. Priestley knew that the experiment which M. Arago has recalled into notice, was erroneous in all its parts; and as it could tend only to mislead, when he reprinted in 1790 the paper which had contained it, he exercised a sound discrimination, and whilst he retained the account of those experiments in which he imagined that he had converted water into common air, as, though erroneous, containing something instructive, he omitted this as utterly worthless. It remains for MM. Arago and Dumas to follow his example of candid retractation, and restore the experiment, with the claims that have been founded upon it, to the grave from whence it has been disinterred.

Having now disposed of two of the points on which I promised to satisfy M. Arago, namely, the worth of Priestley's experiments on the composition and decomposition of water, and of the deduction from them of the theory, doctrine, or hypothesis of Watt, (except, indeed, so far as M. Arago may coincide with my friend Lord Brougham in thinking Watt's introduction of the matter of heat an improvement on the views of Cavendish)*, I now turn to the real history of this great discovery, to which, since the meeting of the Association at Birmingham, I have been enabled to make an addition that cannot fail to excite the interest of all who pay attention to experimental science.

It is one of the privileges of genius to give duration even to its perishable remains. In the expectation that I should find the MSS. of Cavendish still in existence, I applied to Lord Burlington for information, and found that they had passed into the hands of his grandfather, Lord George Cavendish, and thence to the present Duke of Devonshire, from whom I have obtained permission to use them for the elucidation of the present question.

These carefully preserved MSS. exhibit the footsteps of a mind

* Had Watt remarked only that there was here another instance of the general fact, that whenever a combination is formed of greater density than the combining bodies, heat is generated, the remark would have been *just* and *obvious*; but in representing the phænomena as influenced by chemical affinities of heat, and stating that *"dephlogisticated water has a more powerful attraction for phlogiston than it has for latent heat"* (Phil. Trans., vol. lxxiv. p. 334), he introduces a principle which chemistry has no means of investigating.

that had travelled over the whole range of natural philosophy, and they are impressed with many striking marks of undisplayed knowledge, and of that indifference to fame which in England is well known to have been a prominent feature in the character of Cavendish, of whom it has been handed down to us that "he* was peevishly impatient of the inconveniences of eminence, detested flattery, and was uneasy under merited praise." The same negligence of publicity belonged to the characters of Newton, Black, and Cavendish, and their claims to their greatest discoveries have only been substantiated by the interposition of their friends.

What curious facts of this kind appear in the parts of these MSS. which relate to mechanical, meteorological, magnetical, and electrical subjects, I must leave to the examination of others; but I will mention those which have occurred to me in looking over the chemical and geological papers.

The name of Cavendish has never been mentioned among geologists, and I apprehend that it will occasion some surprise to those who have most studied the history of geology, to learn that in journeys of the date of 1787 he ascertained, in company with Blagden, by his own personal observations, assisted by those of an almost equally unnamed, but most acute and comprehensive observer, Mitchell, the entire sequence of all the great beds of English stratification, traced by their mineralogical character, position, and dip, from the beds above the chalk down to the slate-rocks.

At a period when nothing had been published on the subject of latent heat, and the knowledge of Black's discoveries scarcely extended beyond the students of his class at Glasgow, we find Cavendish, with no other information respecting them than the report of a single fact†, deducing all the laws of the *generation and destruction of heat* which attend the conversion of elastic fluids into liquids, and liquids into solids, from an independent and elaborate series of experiments which the world has never heard of, adhering, as in his subsequent investigation of the composition of water, to the Newtonian theory of heat, and denying it that materiality and combining property which has marked, down to the present day, the speculations of the school of Black. These experiments include determinations of the specific heats

* Brand's Preface to Supplement to Encycl. Brit.

† This fact was, that "in distilling water or other liquors, the water in the worm-tub is heated thereby much more than it would be by mixing with it a quantity of boiling water equal to that which passes through the worm."—See Appendix, p. 47.

of a variety of substances, such as wax, spermaceti, and mercury, with other metals, and metallic alloys, antecedent by sixteen years to the first published, which I apprehend were those of Wilcke in the Stockholm Transactions. The same MSS. contain also determinations of the tension of vapour at low temperatures in the barometrical vacuum, and an experimental demonstration, and theory, of that excess of temperature in freely boiling water above the heat of steam, on which observations have since been made by M. Gay Lussac and others.

The portion of the MSS. which belongs more directly to chemistry is small, with the exception of the experiments on air, but not less remarkable as regards unpublished labours and hidden treasure. In this point of view is to be considered a series of experiments on arsenic, which bears the date of December 1764, and had been preceded only by the experiments of Macquer on its neutral salt. At that early period Cavendish *had discovered the acid of arsenic**, *had ascertained the relation in which it stands to the oxide and the regulus, and had examined the salts which it forms*, with at least as much accuracy as Scheele, in the well-known experiments which he published in 1775; this treatise has been twice transcribed by Cavendish from the original notes, in the form of a communication to a friend, and is in a state fit for the press; nor can I conceive any reason for the suppression of experiments of so much value, but that which the character ascribed to him by his friends suggests.

Among these arsenical experiments appears the first statement of the nature of nitrous gas, as he afterwards described it in his paper on factitious airs in 1766, and which was the legitimate statement, till the phlogistic hypothesis was discarded: he had observed also the distinction between nitrous gas and nitrous acid vapour, but was unable to assign the reason of the difference. It further appears from a manuscript among his early papers on factitious airs—on which he has written "Communicated to Dr. Priestley"—*that Cavendish was the first who distinguished nitrogen from other kinds of unrespirable and incombustible gases, and proved by experiment that atmospheric air consists of two parts, one of which in the combustion of charcoal is converted into fixed air, whilst the other is a mephitic gas sui generis*: Priestley, in a paper in the Philosophical Transactions for 1772, mentions this communication, but has not stated correctly the conclusions which it contains.

As remarkable an instance as any which I have observed of Cavendish's habit of keeping discoveries in abeyance, especially such as he

* Appendix, p. 52.

had not completed to his entire satisfaction, is to be found in a MS. constituting a "*4th part*" of his celebrated experiments on factitious air. This unpublished paper, written for the Royal Society, probably in 1766 or 1767, and consisting, with a "*digression on air*" which accompanies it, of twenty-six pages, commences with a series of experiments on the air produced from animal and vegetable substances in distillation, the animal matter employed being hartshorn shavings, the vegetable substances, wainscot and tartar: the inflammable gases from these he finds nearly similar to each other, but so different in specific gravity, and *explosive power*, from the inflammable gas yielded by metals, that he determines them to be of a different kind: he then examines the caput mortuum of the distillation by deflagrating it with nitre, and finding, contrary to expectation, that the weight of the fixed air produced is greater than that of the charcoal consumed, leaves off abruptly, in doubt whether the experiment is incorrect, or whether part of the fixed air is to be ascribed to the nitre. We see from the statement which I have mentioned as having been communicated by him to Dr. Priestley before 1772, that by that time he had acquired clearer ideas of the generation of fixed air.

From this account of experiments which Cavendish never chose to publish, I pass to those of which he delayed the publication till he had completed them, though in their progress he made no secret of them to his friends. They fill a volume of unsewn and single, but paged and indexed octavo sheets, in his own hand, bearing dates from February 1778 to May 1785, in the following proportions: in 1778 thirty-three pages, in 1780 thirty-six, in 1781 seventy-five, in 1782 forty-five, in 1783 fifty-three, in 1784 forty-four, in 1785 thirty-three. I found them in a packet entitled "Experiments on Air."

These very numerous and laborious experiments all bear on the solution of one question, namely, what becomes of the lost air in the various chemical processes, in which it is now known that oxygen passes into fixed combinations? Scheele in 1777* had stated this question thus: "It appears in the transition of what is called *inflammable principle* into the air a considerable part of the air is lost. But whether the phlogiston which is lost from the substance still remained (in his before-mentioned experiments) in the residuum of air in the bottle, or whether the lost air was united and became fixed with the substances of liver of sulphur, oils, &c., is a question of great importance. It would follow on the first supposition that phlogiston has the power of de-

* Scheele's work on air was written in 1775, but not published till 1777.

priving air of part of its elasticity, and that for this reason it is more compressed by the external air. In order to extricate myself from these doubts I first supposed that such air ought to be specifically heavier than common air, both on account of the phlogiston it had gained, and its greater density. But how great was my astonishment upon finding that a very thin retort filled with this kind of air and weighed in the nicest manner, was not only not heavier than an equal measure of common air, but even somewhat lighter! I then imagined that the last supposition might be applicable, and then it would follow that the lost air might again be separated from the materials employed in the experiment." With this view he tries whether "the lost air had not been changed into fixed air," and failing in finding any, as the next resource, alludes to the idea that "the phlogiston when united with this air might make it less ponderous;" "however, since phlogiston," he adds, "is a substance (which always supposes some weight), I very much doubt whether this hypothesis is founded on truth;" and he concludes at last by saying, "I will prove that by union of air [oxygen] with the inflammable principle (phlogiston) a compound is formed so subtle as to pass through the fine pores of the glass and disperse all over the air." This subtle compound was the matter of heat and light: in the shape of these incoercible and imponderable agents, oxygen, and the elements which enjoyed the title of phlogiston, became free to pass to and fro unquestioned; and thus Scheele cut the knot which he was not able to untie. He afterwards informs us that "this generation, or new composition of heat and light by an union of air of fire with more or less phlogiston, obtained" not much applause; "but could I," he adds, "naturally conclude otherwise, when I saw a mixture of air of fire and inflammable air after its explosion totally disappear, but that the fire air, when united with the inflammable, must have penetrated through the glass? because nothing could be observed on the outside of the glass in which the explosion was effected but heat and light; also in the water over which it was effected in close vessels, notwithstanding I varied the experiment in different ways, I could perceive nothing uncommon." "I had often burnt a mixture of dephlogisticated and inflammable air, and have always observed a dew in the glass immediately after the explosion; but I believed that air always contains some moisture and that the inflammable air might also contain some moisture from the vitriolic acid, nay, that even the flame of the candle may yield some dampness*."

* Crell's Chem. Annals, 1785, vol. i. part 3, p. 229.

Precisely the same difficulties had been previously felt and remarked by Priestley*, and he had sought for a partial solution of them in the precipitation of carbonic acid which had occurred in some of these processes, and its absorption by the liquid over which the diminished air was confined. This was the prevailing opinion of the cause of the diminution; Bergman, Kirwan, and other eminent chemists adopted it, paying little attention to the new views of Lavoisier, who already in 1776 had found the key by which these phænomena were ultimately to be interpreted, and had succeeded in deciphering many of them: but Lavoisier had at the same date deduced from too narrow an induction the theory which so long continued to prejudice the chemists of his school, that oxygen is the principle of acidity, and was unprepared in consequence to conceive that it could be one of the constituents of water†.

In this state of the subject Cavendish took up those parts of the problem which Lavoisier had not already sufficiently solved: he began, in 1778, by ascertaining that when nitrous gas is mixed with oxygen or common air, no carbonic or vitriolic acid is produced, but, as Lavoisier had said, nitric acid alone; and he then commenced those eudiometrical researches which were the foundation of all his subsequent discoveries.

In 1779 he appears to have intermitted his experiments; but he resumed them in 1780, and obtained in this year so complete a mastery over the methods of analysing atmospheric air, as to have determined the proportion of oxygen in it to be $\frac{10}{48}$, at a time when Scheele and Lavoisier supposed it to be $\frac{1}{4}$, and Priestley nearly as much. By the same means he had acquired also the power of detecting the smallest adulteration of oxygen gas, and the amount of the impurity; and he came therefore, in July 1781, to the question *what becomes of the*

* In 1774 Priestley writes, "In what manner air is diminished by phlogiston, independent of the precipitation of any of its constituent parts, is not easy to conceive; unless air thus diminished be heavier than air not diminished, which I do not find to be the case. It deserves, however, to be tried with more attention. That phlogiston should communicate absolute levity to the bodies with which it is combined is a supposition that I am not willing to have recourse to, though it would afford an easy solution of the difficulty."

† "*L'analogie,* dit il (Lavoisier, Mém. de l'Académie 1781, page 471), *m'avoit porté invinciblement à conclure que la combustion de l'air inflammable devoit également produire un acide.* Mais M. Lavoisier a senti que toutes les analogies doivent disparoître devant des faits positives; et qu'il y a, entre les analogies les plus fortes et les faits, la différence qui se trouve entre les probabilités et la certitude."—Consid. sur les Exper. de M. Priestley, par M. Berthollet (Annales de Chimie, tom. 3, 1789).

air that disappears in the combustion of dephlogisticated and of common air, with inflammable gas, enjoying the peculiar advantage of a more intimate acquaintance with all the gases operated upon than any other chemist possessed. Of the oxygen examined by so many chemical tests he knew the quality, and the quantity, whether he used the air of the atmosphere or obtained it from any other source, whilst his constant employment of the test of specific gravity gave him an accurate knowledge of the residual gas: he had ascertained also with care the properties of inflammable gas, and even gone some way, in 1766, towards determining, by attention to the comparative loudness of the explosion, the proportions in which oxygen and hydrogen most perfectly combine*.

These observations may tend to diminish the surprise with which the most skilful and experienced in such researches cannot fail to be struck, when they observe the precision with which Cavendish, as soon as Warltire's experiment† had suggested to his mind an *experimentum crucis*, to determine between the truth of Scheele's supposition and the more probable explanation of what had become of the burnt air, offered by the circumstance of the deposition of water, proceeded without the loss, if I may so speak, of a single move, by a regular gradation of six quantitive trials of explosive mixtures, to solve the question. In the fifth or mean of these (MSS. p. 114) he found *the point at which the entire volume of two of the gases disappeared, leaving the entire volume of the nitrogen of its proper specific gravity and proved by chemical tests to have parted with all its oxygen: the bulk of two volumes of hydrogen and one of oxygen was gone, but*

* Phil. Trans., vol. lvi.

† It was not Warltire, but Volta, who first fired mixtures of hydrogen and air by the electric spark. (See a letter from Volta to Priestley, dated Como, Dec. 10, 1776. Priestley's Experiments on Air, 3d Ed., 1781, App. p. 381.) In 1779 Priestley endeavoured to ascertain the relative proportions of phlogiston in nitrous gas and inflammable air, "by the help," he says, "of that ingenious experiment of Mr. Warltire's mentioned in the Appendix to my 3d volume, p. 367, viz., burning inflammable air in a given quantity of common air." "Of this curious problem, however [the proportions of phlogiston], I obtained a more accurate solution from the mode of experimenting introduced by that ingenious philosopher, M. Volta, who fires inflammable air, in common, by the electric spark, and consequently can determine the exact proportion of inflammable decomposed in a given quantity of common air." Priestley's skill in Eudiometry did not enable him, however, to come nearer the truth than to find the proportion of common air dephlogisticated by inflammable air as 2 : 1.

their weight remained in the vessel: the conclusion therefore which Cavendish drew was infallible, (being a necessary consequence of the indestruction of matter)—" that when mixed in these proportions and exploded, almost all the inflammable air and about $\frac{1}{5}$ of the common air lose their elasticity, and are condensed into the dew which lines the glass *."

The subsequent laborious comparisons instituted by the French philosophers † between the absolute weight of the gas consumed, and the weight of the fluid produced, added nothing to the certainty of this proof of its composition, nor even to the accuracy of our knowledge of the proportion of its constituent parts; they took the only means from which in such a method any accuracy can be expected; operating with immense quantities of gas, at great cost, with much expense of time and toil, and taking all imaginable precautions, they yet never obtained a weight of water exactly equal to that of the gases, though near enough undoubtedly to satisfy those to whom such a mode of proof was more familiar than that devised by Cavendish; but every chemist knows that the method of volumes, first introduced on this occasion by *him*, is the proper method of pneumatic determinations.

Thus far had he advanced in the early part of July 1781 (MSS. p. 113). One more experiment, so contrived as to enable him to consume a large quantity of the gases, sufficed to prove *that the fluid condensed was pure water*; and thus, on one of the latter Sundays of that month, (MSS. p. 127, foot note,) the general fact of the composition of water was completely established.

In August (MSS. p. 120) he examined by the test of similar experiments whether there was any difference in the hydrogen furnished by *zinc*, or *iron*, and found none; he contrived also an instrument for measuring the force of the explosions (MSS. p. 130), and appears to have used it as a test of the identity and purity of the gases, as well as of their combining proportions, though its indications were not precise enough to induce him to mention it in his paper. In these experiments on the force of explosion he employed oxygen as well as atmospheric air; and proceeding in September to collect the fluid produced by the combustion, found on the 28th of that month, 1781, (MSS. p. 146,) that *the fluid produced by exploding two volumes of hydrogen with one of oxygen contained nitric acid.*

* Phil. Trans. 1784, p. 128.

† The first proof which Lavoisier gave of the composition of water was the same as that given by Cavendish; *and he deduced the equality of the weights as a consequence or corollary from this proof.*

This oxygen having been obtained from nitrate of mercury, he supposed that the nitric acid might have been derived from the gas, and therefore repeated the experiment with oxygen disengaged from red precipitate by oil of vitriol; but he still found nitric acid.

Such were the experiments made in 1781, " concerning the reconversion of air* into water by decomposing it in conjunction with inflammable air", which Priestley† and Cavendish‡ mention as having been communicated to the former, and repeated in consequence by him in April 1783.

In the following year, 1782, Cavendish made further experiments on the analysis of air, and the tests of oxygen, and in October resumed the investigation of the cause of the production of nitric acid in the combustion of that gas with hydrogen: he now employed the oxygen evolved by plants, under the action of solar light, with the same result; but varying the proportions of the gases (MSS. pp. 203–5.) discovered that *an excess of oxygen* conduced to the production of the nitric acid: he had probably before conjectured the cause of the phænomenon, and in his next experiment, in January 1783, (MSS. p. 211,) he *added a little nitrogen* to the excess of oxygen, and found the quantity of acid still further increased; but when he mixed nitrogen with oxygen (MSS. p. 217,) *in the proportions of common air*, and exploded either this mixture, or common air, with a quantity of hydrogen insufficient to consume all the oxygen in it, the excess of the latter no longer determined the formation of acid, but, as in his first experiments, pure water was the result. Hence he came to the following conclusions—that when hydrogen is burnt with oxygen, slightly contaminated with nitrogen, and in excess, the excess of oxygen forms with the nitrogen nitric acid§; but that when it is burnt with oxygen mixed, as in common air, with a large proportion of nitrogen, *the heat of the*

* That Priestley by air, here means oxygen, which was often so called *κατ' ἐξοχην*, appears from his manner of repeating the experiment, and from Watt's defective acknowledgement, (the only notice he takes of any of Cavendish's experiments,) "I believe that Mr. Cavendish was the first who discovered that the combustion of *dephlogisticated* and inflammable air *produced moisture, on the sides of the glass in which they were fired.*" Cavendish does not (as M. Arago says) *insinuate*, but states distinctly, and without contradiction, that he mentioned to Priestley " *all his experiments* on the explosion of inflammable with *common* and *dephlogisticated* airs, except those which related to the *cause* of the acid found in the water." Phil. Trans. vol. lxxiv. part 1. p. 134.

† Phil. Trans. 1783, p. 426. ‡ Ibid. 1785, p. 134.

§ Experiments on Air, Phil. Trans. vol. lxxiv. part 1. p. 139.

explosion is so much diminished, that though the affinities of *hydrogen* and oxygen are sufficient to determine at that temperature the formation of *water*, the affinities of *nitrogen* and oxygen are not sufficient to determine the production of *nitric acid**.

These were the last and the only experiments which Cavendish ever made on the combustion of hydrogen and oxygen; he had completed the investigation, and reverted no more to the subject. Few rough day-books of experiments would tell their own tale with such certainty and distinctness as these; in few could the consecutive course of reasoning be traced thus clearly from the experiments themselves: there cannot remain a doubt on the mind of any one who reads them, that in January 1783 Cavendish had not only discovered the certain fact that oxygen and hydrogen in definite proportions form water, but likewise the strong probability that oxygen and nitrogen form nitric acid, two months before Priestley began to experiment, and Watt to speculate, on the notice which Cavendish had given the former of the composition of water, and four months before Lavoisier received from Blagden a similar notice.

These experiments were followed by an analysis of the gas distilled from charcoal, which it is probable from some expressions in his paper he may have been led to make by the circumstance of Priestley's having used this gas in repeating his experiments without noticing the production of nitric acid†. The quantity of oxygen which he found consumed in its combustion corresponds precisely with Dr. Henry's determination; the quantity of carbonic acid appears to be less, probably from his manner of estimating it, by weighing the carbonate of lime precipitated, instead of measuring the gas. At the same time, that is to say in September 1783, it appears from these MSS. that by burning nitre with charcoal, and effecting a total decomposition of the nitric acid, he confirmed analytically his synthetical discovery of its composition.

In 1784 he proceeded to investigate the diminution effected in air by the electrical spark, and found, on examining the amount and product of the condensation, not only a further confirmation of the composition of nitric acid, but the means of discovering, by the same method of

* Experiments on Air, Phil. Trans. vol. lxxiv. part 1. p. 134.

† "It is remarkable that neither of these gentlemen (Priestley and Lavoisier) found any acid in the water produced by the combustion, which might proceed from the latter having burnt the two airs in a different manner from what I did, *and from the former having used a different kind of inflammable air, namely, that from charcoal.*" Phil. Trans. vol. lxxiv. part 1. p. 135.

volumes as before, *the combining proportions* of nitrogen and oxygen. One of these experiments well illustrates the rigorous precision of his chemical ideas. It is difficult to bring the whole of a given quantity of nitrogen into combination by the electric spark; in the preceding experiments the proportion of that gas which had disappeared with the oxygen had been observed; but there was *a residue*; and while that remained Cavendish was dissatisfied with the evidence: we know nitrogen, he argued, only either by *negative* properties—that it does not burn—that it does not support respiration,—or by the *relative* property of its specific gravity: these properties cannot give an absolute certainty that it is not a *mixed* gas: its *entire combination* can alone prove that it is *unmixed*: he therefore devised an experiment by which he contrived to convert *the whole of the quantity operated upon* into nitric acid, and thus he established with respect to it, what he had established before respecting hydrogen and oxygen, the fact of *its simplicity as a combining body*. What I mean by *its simplicity as a combining body*, will be understood by observing that he continues to use just the same hypothetical language respecting nitrogen as before, and still speaks of it as *a compound of nitric acid and phlogiston*. On his first discovery of the composition of water he had entrenched the old doctrine of phlogiston in the only position which was any longer tenable: he fought the same battle for it as was fought in later days for oxymuriatic acid, and on the same grounds: as I have shown that whenever *Cavendish* used the term dephlogisticated air, oxygen may be safely substituted for it, and whenever he uses the term phlogiston, hydrogen* may be substituted for it, I am entitled to show the similarity of the reasoning in these cases more distinctly by explaining it in modern terms: "As *dephlogisticated air* [*oxygen*]", he argues, "*is only water deprived of phlogiston* [*hydrogen*], *it is plain that adding dephlogisticated air* [*oxygen*] *to a body is equivalent to depriving it of phlogiston* [*hydrogen*] *and adding water to it, and therefore phlogisticated air* [*nitrogen, supposed a compound of dry nitric acid and hydrogen*] *ought also to be reduced to nitric acid by being made to unite to, or form a chemical combination with dephlogisticated air* [*oxygen*]; *only the acid formed this way will be more dilute than if the phlogisticated air was simply deprived of phlogiston* [*hydrogen*] †."

Thus did Cavendish, in deference to received opinion, speak of nitrogen as a compound, while at the same time he was taking pains to

* It is worthy of remark that Cavendish was the first chemist who identified phlogiston with hydrogen. Phil. Trans. vol. lvi. 1766.

† Phil. Trans. 1785, p. 379.

prove that it enters undecompounded into composition with other bodies. Having proved this, it would have been better if he had dropped the term phlogisticated air: but though the language is hypothetical, the ideas are precise: and with respect to hypothesis, a distinction must be made between the art of communicating, and that of discovering truth: I have noticed in my Address how important it is, for the sake of clearness, and for the avoidance of prejudice, to discard from our reasonings all hypothetical expressions, resting, like this of the supposed combinations of phlogiston, on loose analogies. Nevertheless in the mind of every discoverer a private reserve is made for the admission even of loose analogies, and for the idea that every body deemed simple may prove to be compound; previous, for instance, to proof of the composition of the alkalies, to have spoken in hypothetical language of the oxide of potassium, would have been logically objectionable; but yet if Davy had not entertained the hypothesis, he would never have made the discovery.

The remaining experiments in this manuscript, bearing dates of 1784–5, are almost entirely devoted to the combustion of charcoal and of the gas distilled from it, in various ways, with common air, oxygen, and nitre, the chief object of which seems to have been to ascertain whether that body is a compound. The conclusions at which Cavendish arrived were, that it contains no nitrogen, but that there was reason to suspect that it might contain some hydrogen.

His computation of the experiments which he had made in 1783 on the gas distilled from charcoal is worthy of remark. It was not known, till Cruikshank made the discovery in 1801, that carbon and oxygen unite in other proportions than those which form carbonic acid; nor was it known, till Henry stated it in 1805, that carbonic oxide, or oxygen in any form, existed in the gas from moist charcoal. Cavendish however deduced the last of these facts from his experiments; he computed this gas to consist of carbon, hydrogen, and *water*, stating the water at such an amount, as fully to represent the amount of oxygen in the gas*, and to account for the presence of free hydrogen without the corresponding oxygen which would have been due to the decomposition of water, he proposes two suppositions—" From this ex-

* It was probably from these experiments that Cavendish derived in part his opinion before alluded to, that water is a constituent part of inflammable air; since it appeared to be so in that kind which is obtained from charcoal. It is remarkable, that Cruikshank, after his discovery of carbonic oxide, should have overlooked its existence in this gas, and like Cavendish, should have computed the gas to contain water, in the proportion of 9 grains in 14½.

periment it should seem either that charcoal contains hydrogen, or else that the charcoal after distillation contained some oxygen."

Speaking of the latter gas in this computation Cavendish uses all the various terms, *dephlogisticated air*, *pure air*, and *oxygen*: I have given in the Appendix a letter in which he assigns to Blagden his reasons for disapproving of the introduction of a nomenclature entirely new; and unquestionably the term *oxygen* was open to the same objection with the old name of dephlogisticated air, as equally involving an erroneous hypothesis: Cavendish shewed in his paper on air that oxygen does not always acidify; and it has since been proved that it does not exist in all acids: he objected to the new language, that without the advantage of being already understood, it involved, no less than the old, theories which in the rapid progress of chemical discovery might quickly be disproved: he thought the time for such a reformation of terms not yet arrived: he was mistaken; for with the new language a new and more rigorous system of reasoning was introduced into common use, and by degrees chemists learnt from experience to correct also their notions of nomenclature, and discovered that in expressing undecompounded substances, the less significant is the name, the better it serves the objects of science.

Having now concluded my analysis of the chemical contents of these MSS., I think it right to enable every one to judge for himself, by lithographing *the whole* of Cavendish's experiments on the composition of water, and by giving a few extracts from those on other subjects. I request the attention of M. Arago in particular, and of M. Dumas who has adopted his views, to a letter addressed by Cavendish in February 1785 to the editor of the *Journal de Physique**, which will show them how incapable he was of falsifying dates, or attempting to reap fame from the mistake of a printer. It is to be regretted that Watt did not take equal care to prevent the propagation of such mistakes. When Dr. Black's lectures were published after his death, with a dedication to Watt, he would have done well not to have allowed the history which is there given of the discovery of the composition of water to pass without correction; almost every statement and every date in that history is a mistake, and the effect of those mistakes is to give precedence to Watt†. Independent of other errors, the whole experiment

* Appendix, p. 65.

† The following is the account of this discovery given in Black's Lectures as edited by Robison, with a dedication to Watt, in 1803. The errors are printed in Italics.

"Dr. Priestley was occupied with the examination of inflammable air, and

ascribed in this account to Cavendish, *with its date of May* 1783, is, from the beginning to the end, as to its existence, and all its details, a fiction: it is easy to conceive a confusion of recollection which might lead a lecturer to substitute his own ideas of what an experiment might have been for what it was, or to confound the experiments of different persons on the same subject, though this is scarcely excusable where the statement is one professing to adjust their respective claims. Let us suppose then such a confusion to have led to the ascription to Cavendish of the experiments of the French chemists; but how shall we account for the particularity of the *fictitious date of May* 1783 for the time of Cavendish's discovery? Cavendish had not said a word about May; he had said that the experiments which constituted his claim to that discovery were made in the summer of 1781; he had not

tried the effect of almost every substance on it: he also tried the effect of the electrical shock and spark. As he expected, he found that it was expanded by it, but could not be inflamed by it in close vessels, unless mixed with common air. In this state it fired with a violent explosion. He was particularly surprised at the great diminution of bulk,—finding that a mixture of one part of inflammable air, and two of common air, might be made to contract into half the bulk, and that it was now phlogisticated air. Having already discovered the vital air, he fired a mixture of these, and found that *when two parts of inflammable air and one of vital air were exploded together, it collapsed into almost nothing, or nearly the whole disappeared.* Mr. Warltire, who assisted in these experiments, observed that the inside of the vessel in which the deflagration had been made, was always moistened with dew. Dr. Priestley naturally ascribed this to moisture, which probably adhered to the airs employed, as they were always produced in processes in which water in some form or other was present. *These experiments were made about the year* 1782. [1781 is the true date both of the experiments really made by Priestley and Warltire, which include no such fact as that alleged above, and those of Cavendish here confounded with them.]

"My friend Mr. Watt had taken great interest *in these experiments* of Dr. Priestley's, and communicated his opinion concerning them *to M. de Luc, in a letter dated April* 1783. [The true date of the experiments, here confused with those of 1781, is April 1783, that of the letter is Nov. 1783.] This letter is, in part, a transcript of one written some months before to Dr. Priestley, with a desire that it should be communicated to the Royal Society. [The true date is April 1783.] In this he declares his opinion, that the water observed in these experiments arose from the combination of two airs; and says that water is the compound of dephlogisticated or vital air, and inflammable air, deprived of their latent heats; and that dephlogisticated air is water deprived of its phlogiston (i. e. of the inflammable air), in an aerial form, that is, saturated with the matter of light and heat. Dr. Priestley did not communicate this to the Society, *because (he says) some experiments which he had made since*

made a single experiment on the subject after February 1783. Why then the date of *May* in that year? I know not, unless because Watt claimed to have made the discovery in *April* 1783, and Lavoisier claimed to have made it in *June* 1783; and *therefore* by the *argument from kindness and country*, which sometimes gains an ascendence over the imagination and belief even of the wisest and most virtuous minds, Cavendish *must* have made his decisive experiment in the intermediate month of *May*. There can be no doubt however that had these lectures been printed during the life of their author, he would have taken more pains, before he had published them, to ascertain the facts. Nor ought it to be omitted, that in other parts of the volume the discovery of the composition of water is assigned to its rightful owner, and that

he saw Mr. Watt, were directly contrary to this opinion. [Mr. Watt himself withdrew the communication.] *Dr. Priestley's experiments excited the attention of the Honourable Mr. H. Cavendish, and recalled to his mind his own observation of the moisture in the vessels in which he had exploded these two airs.* [There is no foundation for this statement.] These experiments had been begun in the summer of 1781, and were continued from time to time, along with those by which he had discovered the composition of the nitrous acid. *He immediately set about repeating the explosion of dephlogisticated and inflammable airs by the electrical sparks*; *and in May* 1783, he found that when *six parts by weight* of pure dephlogisticated air were exploded with one of inflammable air, they disappeared entirely, and that the result was, a quantity of pure water, *equal in weight to the airs employed. The utmost care had been taken to free the airs made use of, by making them pass through the dry muriat of lime.* [These were the experiments of the French chemists.] *The vessel burst in several of his experiments, because, in the instant of explosion, the vapour of the produced water was expanded by the heat extracted from the airs. Much of this heat, to be sure, was expended in giving these the vaporous form, or supplying it with latent heat. But the vessel was instantaneously heated, shewing that the heat contained in the two airs more than sufficed for this purpose. These experiments were published in the Philosophical Transactions for* 1784. [No such observations were made by Cavendish or published in the Transactions.]

"Such curious experiments, and so interesting a result, could not remain a secret, had such a thing been intended. But there was no such intention. Mr. Blagden, Secretary of the Royal Society, went to Paris in June 1783, and communicated these experiments of Mr. Cavendish to M. Lavoisier, and his associates, De la Place, Meusnier, Mongez, &c., knowing that they were much interested in the result, which was so intimately connected with the new theory which M. Lavoisier was then establishing.

"Accordingly, M. Lavoisier, who saw the immense consequence of this discovery to his theory, immediately set about repeating the experiments of composing water by the combination of the two airs; and in September 1783, with the assistance of M. Meusnier, effected the composition in a way that admitted no doubt."

the errors in this account are such as to exonerate Watt from any suspicion of having supplied or revised it.

The fault which, since the subject has been forced into public notice, we are compelled not to leave unobserved in the conduct of the latter, is the silence with which he passed by the experiments of Cavendish in his letters both to Priestley and De Luc. How fully Priestley's acknowledgment, that Cavendish had made before him the same experiments, was understood by others, appears from the following abstract of the contents of Priestley's paper on the seeming conversion of water into air, which I have extracted from the Journal Book of the Royal Society. "These arguments received no small confirmation *from an experiment of Mr. Cavendish,* tending to prove the reconversion of air into water, in which pure dephlogisticated air and inflammable air were decomposed by an electric explosion, and yielded a deposit of water equal in weight to the decomposed air." This abstract, which shows how ill-founded the story is of the discovery of the composition of water being received with ridicule by the Royal Society, was made by the Secretary of the Society immediately after the reading of Priestley's paper in June 1783. The Secretary was Mr. Maty, not Dr. Blagden, who did not hold the office of Secretary till May 1784, and was not a member of the Council; so that he is in no way liable to the suspicion intimated by Lord Brougham of having shown Watt's letter to Cavendish, nor to the reproach which M. Arago casts upon him, of not speaking the whole truth respecting the precise date at which Watt's opinions were made known in London. The fact is, that there is a good deal of confusion attending this letter of Watt's; the date with which it is printed is April 26; but that date is corrected in the MSS. in two places to April 21. Watt says it was received by Priestley in London; and yet it is certain that Priestley transmitted it to Sir J. Bankes, with his own paper, from Birmingham: its date however is of no consequence.

The perusal of the entire letter has satisfied me that the real ground of Watt's claim to original views on this subject was not the observation of what he calls "*the obvious fact,* that inflammable and dephlogisticated air unite with violence, and that water is the only fixed product," but the theory which he engrafted on this fact, *that the airs are decomposed, and that their bases, parting with the elementary heat with which they were combined, enter into a new combination, and so form water*—a theory which Lavoisier had applied generally to the other phænomena of combustion six years before.

M. Arago, however, might have found for the subject of his *Eloge* a higher ground of *scientific* praise in less questionable applications of *the*

theory of latent heat. He has poured the whole force of his eloquent panegyric on the *practical results* of the labours of the great engineer, and the *mechanical ingenuity* displayed in them; but he might have pointed to the early and profound attention which he devoted to this theory, as the fountain-head of his fame, and shown him in the academical class room, and the laboratory, of Black, imbibing that spirit, and advancing those principles of science, which were afterwards embodied in all his works, and gave life to all his inventions; and he might thus have taught us, in a manner worthy of his own genius, what it is more especially, that entitles Watt, rather than Worcester or Papin, Savery or Newcomen, to be admired as the *philosophical parent* of the gigantic and diversified powers of the steam-engine.

APPENDIX.

EXTRACTS FROM THE CAVENDISH MSS. REFERRED TO IN THE FOREGOING POSTSCRIPT.

EXPERIMENTS ON HEAT.

[When an edition of the works of Cavendish is published, (as I believe is intended), I hope that these papers on heat will be printed at large with all the experiments. I have here confined myself to extracting such passages as are sufficient to substantiate what I have advanced in the Postscript to my Address. The treatise containing these views, and the general result of the experiments, was written for the use of some individual, it does not appear whom. The same is the case with respect to the experiments on arsenic, which follow: and the date of both these remarkable series of experiments appears to be about the same, that is to say, certainly not later than 1764: as with respect to those on heat the following extract, copied *verbatim* from the original notes (p. 89) of the experiments, shews.

"Feb. 5, 1765:—therm. in room about 35: heat of liquors in bottles, 34: the weight of the bottles with the liquors was known: the liquors were put into wide-mouthed vessels inclosed in wool, the *weight* of the glasses being known; & snow added till therm. sunk to about 19: the snow seemd not at all inclinable to melt when taken up, & was put in glass vessel set in mixture of snow & salt water: the heat of snow when made use of was neglected to be tried: the solution of sea-salt sunk to 18½, the spt of wine to 19, the f. alk. to 22, & the aq. fort. to 19: the spt of salt was not tried. After the experiment was finished, the wide-mouthed glasses with

the liquors in them were weighd, & also the bottles which the liquors were poured out of; by which means the quantity of liquor was known, & also the weight of the snow; the result is on the other side the leaf: the small bottles were not tried."

This entry stands in his note book subsequent to all the entries of experiments on the heat of mixtures of "hot & cold water"—"hot water & quicksilver"—"hot quicksilver & cold water"—"hot quicksilver & cold quicksilver"—"hot water & oil"—"hot quicksilver & cold spts"—"hot spts & cold spts"—"hot spts & cold quicksilver"—"hot quicksilver & solut. pearl ashes"—"hot quicksilver & oil vitr."—"silver sand, iron filings, lead shot, powdered glass, powdered marble, tin shot, powdered charcoal, Newcastle coal, brimstone, tried in tin bott. in hot water"—"cold spermaceti in warm water"—"concerning heat & cold produced by hardening & melting of spermaceti"—"expts on time of evaporation of boiling water"—"exper. concerning the cooling of water in worm-tub, by blowing air through pipe, & concerning the heating of it by distillation";—with the following result of the latter experiments.—"*Therefore heat gen. by condens. vapours* = 942°."—MSS. p. 71.

At what date Black and Watt arrived at a similar result I know not. Nor do I know the precise year in which Black first taught the doctrine of specific heat. Dr. Thomson says, "That the specific caloric of bodies is different, was first pointed out by Dr. Black in his lectures at Glasgow between 1760 & 1765. Dr. Irvine afterwards investigated the subject between 1765 and 1770 (Black's Lectures, i. 504), and Dr. Crawford published a great number of experiments on it in his Treatise on Heat (1779), but Professor Wilcke, who published the first set of experiments on the subject (Stockholm Transactions, 1781), introduced the term specific caloric." "I have been informed", he adds, "by the late Professor Robison, that Wilcke's information was got from a Swedish gentleman, who attended Dr. Black's lectures, about 1770." It appears probable, from what I have stated, that this unpublished series of experiments by Cavendish is the first made upon this subject. After these, and immediately preceding those of the date Feb. 1765, is Cavendish's determination of the number of degrees of "cold gen. by thawing ice or snow," which he found on an average to be 150°. In the account which Black gives, in his Lectures, of his determination of the quantity of heat absorbed in the melting of ice, he says, "these two experiments and the reasoning which accompanies them were read by me in the Philosophical Club or Society of Professors of Glasgow in the year 1762." The following is the only notice which Cavendish has given of any of his numerous and elaborate experiments on these subjects. In his "Observations on Mr. Hutchins' experiments for determining the degree of cold at which quicksilver freezes" (Phil. Trans. 1783.), he says,

"The cause of the rise of the thermometer when the water begins to freeze, is the circumstance, now pretty well known to philosophers, that all, or almost all, bodies, by changing from a fluid to a solid state, or from the state of an elastic to that of an unelastic fluid, generate heat; & that cold is produced by the contrary process. This explains all the circum-

stances of the phenomenon perfectly well." "I formerly found, by adding snow to warm water, & stirring it about till all was melted, that the water was as much cooled as it would have been by the addition of the same quantity of water, rather more than 150° colder than the snow, or in other words, somewhat more than 150° of cold are generated by the thawing of snow, & there is great reason to think that just as much heat is produced by the freezing of water. The cold generated was exactly the same, whether I used ice or snow. (Note.) I am informed that Dr. Black explains the above-mentioned phenomena in the same manner, only instead of using the expression 'heat is generated or produced,' he says, 'latent heat is evolved or set free'; *but as this expression relates to an hypothesis depending on the supposition that the heat of bodies is owing to their containing more or less of a substance called the matter of heat; & as I think Sir Isaac Newton's opinion, 'that heat consists in the internal motion of the particles of bodies,' much the most probable, I chose to use the expression 'heat is generated.'* Mr. Wilcke also, in the Transactions of the Stockholm Academy of Sciences, explains the phenomena in the same way, & makes use of an hypothesis nearly similar to that of Dr. Black. Dr. Black, as I have been informed, makes the cold produced by the thawing of snow 140°; Mr. Wilcke, 130°."

He then goes on to mention briefly that he had formerly kept a thermometer in melted tin and lead, &c. And this is all he has said of experiments, made 19 years before, the notes of which fill 120 pages, 8vo., and the paper of results and deductions from them, 50 pages, 4to.: the following extracts will give a general idea of the contents of the latter, and of a fragment which seems to have been written still earlier.]

FRAGMENT ON HEAT.

Hypothesis.

All bodies, in changing from a solid state to a fluid state, or from a non-elastic state to the state of an elastic fluid, produce cold, & by the contrary change they produce heat.

The principal cases in which bodies are changed from a non-elastic to an elastic state, are the evaporation of liquors, & the separation of fixed air from alcaline substances.

Before we consider how far this hypothesis agrees with experiment, it may be proper to premise, that most bodies which have any considerable affinity to each other, generate heat in mixing. This is well known to be the case in many instances: such as mixing oil of vitriol, & the 2 other mineral acids, spirits of wine, quick lime, & dry pearl ashes, with water, & many other instances, though in some, such as the solution of salts in water, there seems to be cold generated, & there seems to be cold produced by the mixture of substances which have an affinity to each other, as in the solution of salts in water, but this will be shown to be owing to another cause. I very much question, indeed, whether there is any real instance of cold being produced by the mixture of 2 bodies which have no affinity to each other.

Case 1st.—*On the Evaporation of Fluids.*

Dr. Cullen has sufficiently proved, in his papers in the Edinburgh Essays, that cold is produced by the evaporation of all fluids. There is also a circumstance daily before our eyes, which proves the same thing, though I do not know that it has hitherto been taken notice of.

It is well known that water, as soon as it begins to boil, continues exactly at the same heat till the whole is boiled away, which takes up a very considerable time; I believe I may say several times as much time as it took up to heat it to the boiling point. No reason however can be assigned why the fire should not continually communicate as much or nearly as much heat to it after it begins to boil as it did when it wanted not many degrees of boiling, & yet during all this time it does not grow at all hotter; this I think shews that there is as much heat lost, or in other words, as much cold produced, by the action of boiling as there is heat communicated to it by the fire.

If no cold was produced by the action of boiling, the water should either grow hotter & hotter the longer it boils, or else it should be entirely converted into steam immediately after it begins to boil.

By this means when the water is heated to the boiling point, then as fast as it receives heat from the fire there is immediately so much of the water turned into steam as is sufficient to produce as much cold as it receives from the fire, so that the water is prevented from growing hotter, & besides the water will not be entirely evaporated till it has received as much heat from the fire as is produced by turning the whole of the water into steam. The foregoing circumstance, I think, shews that all animal & vegetable substances, sulphur, quicksilver, arsenic, & many other metallic & other substances generate cold by being changed into an elastic fluid or vapour; though it cannot be shewn by Dr. Cullen's method that they do so, for in distilling any of these substances it takes up a vast deal of time, after they begin to distil strongly, before they are all driven over, & there is no reason to think that their heat increases much after they begin to distil strongly. In general I think there is great reason to suppose that all the substances whatever, which are capable of being volatilized by heat, produce cold thereby.

Case 2nd.—I have been informed, that in distilling water and other liquors, the water in the worm-tub is heated thereby much more than it would be by mixing with it a quantity of boiling water equal to that which passes through the worm.

PAPER ON HEAT.

P. 1. It seems reasonable to suppose that on mixing hot & cold water, the quantity of heat in the liquors taken together should be the same after the mixing as before, & that the hot water should communicate as much heat to the cold water as it lost itself, so that if the expansion of the mercury in the thermometer is proportional to the increase of heat, the difference of the heat of the mixture, & of the cold water, as measured by the thermometer, multiplied by the weight of the cold water, should be equal to the difference of heats of the hot water & mixture, multiplied by the weight of the hot water; or

the excess of the heats of the mixture above that of the cold water should be to the difference of heats of the hot & cold water, as the weight of the hot water to that of the mixture.

P. 4. But before we proceed to compare this experiment with the rule it was intended to examine, it is necessary to make some corrections.

P. 12. Sect. 2. One would naturally imagine that if cold quicksilver, or any other substance, is added to hot water, the heat of the mixture would be the same as if an equal quantity of water of the same degree of heat had been added; or in other words, that all bodies heat & cool each other, when mixed together equally, in proportion to their weights; the following experiments however will shew that this is very far from being the case.

P. 26. It should seem therefore to be a constant rule, that when the effects of any two bodies in cooling one substance are found to bear a certain proportion to each other, that their effects in heating & cooling any other substance will bear the same proportion to each other.

P. 27. The true explanation of these phenomena seems to be, that it requires a greater quantity of heat to raise the heat of some bodies a given number of degrees by the thermometer than it does to raise other bodies the same number of degrees.

PART II.

P. 33. As far as I can perceive, it seems a constant rule in nature, that all bodies, in changing from a solid to a fluid state, or from a non-elastic state to the state of an elastic fluid, generate cold, and by the contrary changes they generate heat. I shall first consider those cases in which bodies are changed from a non-elastic to an elastic state, or from an elastic to a non-elastic state, & afterwards those in which they are changed from a fluid state, or the contrary.

The reason of this phenomenon seems to be, that it requires a greater quantity of heat to make bodies shew the same heat by the thermometer, when in a fluid, than in a solid state, & when in an elastic, than in a non-elastic, state. It is plain that, according to this explanation, all bodies should generate *as much* cold in changing from a solid, as they generate heat by the contrary change, which as far as I can perceive seems to be the case.

P. 40. I have been informed that Dr. Black has observed that in distilling water, the water in the worm-tub is heated thereby much more than it would be by mixing with it a quantity of boiling water equal to that which passes through the worm. Upon this principle I made some experiments to determine how much heat is generated by converting water from the state of an elastic to that of a non-elastic fluid.

P. 43. Experiments to shew that bodies in changing from a solid state to a fluid state produce cold, & in changing from a fluid to a solid state produce heat.—

P. 45. I made some experiments to determine the quantity of cold produced by mixing snow with the following substances, namely, a solution of sea-salt, pearl ashes, spirit of wine, & aqua fortis. The quantity of cold generated was not very different from that produced by dissolving snow in warm water. I

find also, that cold is generated, by the melting, & heat, by the hardening, of spermaceti. The cold produced by the melting of spermaceti is sufficient to cool a quantity of water equal to it in weight above 70 degrees, & nearly the same degree of heat is produced by the hardening of spermaceti. It was tried by putting cold spermaceti into hot water, & hot spermaceti into cold water.

P. 46. Some tin & lead were melted separately in a crucible, & a thermometer put into them, & suffered to remain there till they were cold. The thermometer cooled pretty fast, till the metal began to harden round the edges of the pot: it then remained perfectly stationary, till it was all congealed: it then began to sink again. On heating the metal, with the thermometer in it, as soon as the metal began to melt round the sides, the thermometer became stationary as near as I could tell at the same point that it did in cooling, and remained so till it was entirely melted. On putting a thermometer into melted bismuth, the phenomena were the same, except that the thermometer did not become stationary till a good deal of the metal was hardened, unless I took care to keep the thermometer constantly stirring about. It then remained stationary till it was almost hardened. I do not know what this difference between bismuth & the 2 other metals should be owing to, except to its not transmitting heat so fast as them. I forbear to use the word conducting, as I know you have an aversion to it; but perhaps you will say the word I use is as bad as that I forbear.

P. 48. All the following mixtures, except the first, differ considerably from the 3 simple metals in the manner in which they harden in cooling, as they begin to abate of their fluidity in a heat considerably greater than that in which they grow hard, whereas in the simple metals I could not perceive any difference between the heat in which they ceased to be perfectly fluid, & that in which they hardened.

P. 49. I think it seems likely that the reason why these mixtures begin to abate of their fluidity in a greater heat than that in which they harden is, that the metals of which the mixture is composed begin to separate as soon as the heat is not sufficient to keep the mixture quite fluid. This is confirmed by the following experiment. The mixture of equal quantities of lead & tin was melted over again & suffered to remain quiet till cold; it was then cut in two. The specific gravity of the upper piece was 8·001, & that of the lower 9·031; so that the upper piece seems to contain much less lead than the lower.

P. 50. *Thoughts concerning the above-mentioned phenomena.* There are several of the above-mentioned experiments which at first seemed to me very difficult to reconcile with Newton's theory of heat; but on further consideration they seem by no means to be so: but to understand this you must read the following proposition.—

EXPERIMENTS ON ARSENIC.

[That the paper containing these experiments, like the former, was addressed to some individual who pursued the same studies, appears from the following expressions: "As I have spun these papers out to a great length, I will not repeat the particulars of this experiment, *which I showed you before:*" and again, "thus mercury may be revived without the addition of any matter

usually called inflammable, *as you tell me you have tried yourself.*" It has been twice written over, and is founded on a note-book of experiments, the date of which is given by the following extract from the 27th page of the note-book, after the experiment (page 10 of the paper) on the solution of arsenic in aqua fortis.

"1·10·0 of purified spts of salt, 2 by measure, diluted with ½ the quantity of water, was put into a piece of flor. flask and heated; 2·8·0 of arsenical liquor was poured into it by degrees whilst hot: it effervd & discharged fumes as usual, it seemed almost but not quite suffice. to saturate the acid: the liquor being evaporated gave crystals of sal Sylvii mixed with nitre. See p. 53, infra, P. 9. 10.

"Dec. 1764. Into some of this arsenical liquor was dropt some spt salt: it caused red fumes & turned the liquor rather blue, but, as well as I can judge, not near so much so as at first."]

Experiments on the solution of Arsenic in f. Alkali.

P. 3. The arsen. is precipitated from these solutions, according to Macquer, by any acid whatsoever, which as far as I have tried agrees with my own experiments: there is however a remarkable difference between the mineral acids in this respect; for if you take any combin. of arsen. & f. alk. in which the f. alk. bears a considerable proportion to the arsen., & dilute it with a moderate quantity of water, & then drop into it some oil of vitr. or aqua fortis, the arsen. is not immediately precip.; but in a day or two the bottom and sides of the glass will be found coated with small crystals of arsen.; part of the arsen. however remains suspended so strongly as not to be separated without crystallizing the neutral salt: whereas if you drop spt of salt into the same solution, or even one diluted with a much larger quantity of water, the arsen. is immediately precip. in white clouds.

P. 4. Arsenic, as was before said, does not begin to efferv. with f. alk. without a greater heat than one can bear one's hand in; it also seems to require a much greater heat than that of boiling water to deprive the alcali entirely of its air. Sulphur also cannot unite to a f. alk. saturated with air without depriving it of some of it, which it is not able to do without a greater heat than that of boiling water; for which reason there is hardly any impregnating the milder sorts of fixed alcalies strongly with sulphur merely by boiling together: with the heat requisite to make liver of sulphur by fusion it is able to deprive the f. alk. of a great deal of its air, as appears from the frothing or efferv. during mixing: it however cannot entirely deprive it of its air by that heat, as the liver of sulphur made by fusion always makes some efferv. with acids, though not so much as the f. alk. by itself.

Experiments on the neut. arsen. salt.

1st process for neut. arsen. salt.

P. 5. 20·0·0 of nitre was well mixed with the same quantity of arsen., & committed to distillation in a retort in reverb., the neck of the retort being luted into a glass tube whose end was immersed into 20·0·0 of a strong solution of pearl ashes; the vapours passed through the f. alk. in the form of red fumes

even in the very beginning of the operation; as it proceeded, there formed a white precip. at the bottom of the vessel which contained the f. alk.: the process was continued for upwards of an hour after the fumes had almost ceased to appear, during all which time the heat was kept up much stronger than at the beginning.

The retort was not red hot, though I should imagine it could not want much of it. The cake of neutral salt remaining in the retort after the vessels were taken down weighed about 30·0·0; 3·5·0 of loose flowers of arsen. were sublimed into the neck of the retort; little or none was found in that part of the retort contained within the furnace, & upwards of 4.4 of moist flowers of arsen. were found in the tube. The arsen. sublimed into the neck of the retort was found no ways to differ from common arsen.

3·18·16 of a solution of dry pearl ashes in equal weight of water were saturated by 2·8·11 of aqua fortis, whose spe. grav. was 1·398: the loss in mixing was 10·9. The operation was performed in a flor. flask, & the aqua fortis was previously diluted with water to avoid too sudden an efferv.: being evaporated it yielded 2·11·19 of dry crystals; therefore 1 part of saltpetre contains ·936 of aqua fortis, ·759 of dry pearl ashes, & ·559 of ditto freed from air.

3·9·4 of arsen. was sublimed into the neck of the retort & tube in the foregoing distillation; therefore there remains 16·10·20 of arsenic in the cake of neutral salt, supposing that no arsen. was carried over in the red fumes, as in all probability there was not: therefore 1 part of neut. arsen. salt contains ·506 of dry f. alk. saturated with air or ·373 of the same alcali deprived of air, & ·551 of arsen. Therefore the proportion of f. alk. and arsen. in the neut. arsen. salt is 1 part of dry f. alk. saturated with air to 1,082 of arsen.

If the cake of salt remaining after distillation be dissolved in a proper quantity of hot water it readily shoots on cooling into crystals, which do not at all grow moist in the air, & require about 3½ times their weight of water to dissolve them.

A solution of these crystals scarcely alters the colour of syrup of violets; if anything, they give it a reddish cast; they turn tournsol paper a brownish-red.

A solution of fixed alcali being dropt into a solut. of these crystals makes an effervescence. The quantity of alcaline solution which must be added before it ceases to effervesce is such, that the dry alcaline salt shall be about $\frac{1}{7}$ of the weight of the neutral arsen. salt, or about $\frac{2}{7}$ of the alcali already contained in the neut. salt.

Chalk or whiting also make a slight efferv. with this solution. Macquer seems not to have taken notice of this phenom., since he says, it seems in all respects a perfectly neutral salt; whereas these experiments plainly show an excess of acid, unless you suppose that the salt he made differed in this respect from mine. There is another point in which we differ, relating to the precipitation made by metallic solutions: he says, that blue vitriol and a solut. of iron in aqua fortis make a precip. immediately on dropping into a solut. of the neut. arsen. salt; but that green vitriol and a solution of copper in aqua fortis do not make a precip. till after having stood some time: whereas I have always found that all 4 substances make a precip. the instant they are dropt in.

Experiments on the fixed alcali through which the red fumes were made to pass in the second process for making neut. arsen. salt.

P. 8. The contents of the vessel after distillation were found to weigh 19·7·0; so that the f. alk. appears, instead of increasing in weight, to have lost about 13 parts by the operation: these contents were put in a bottle, & set in sand, in order to dissolve the sediment which, as was before said. fell to the bottom during the process: the bottle broke, but the liquor was saved by the sand-pot; it was washed out from the sand by boiling it with fresh parcels of water; it weighed 36·0·0: into some of this liquor was dropt oil of vitr. diluted with about 3 times its weight of water; it effervesced, & discharged a great quantity of red fumes like those produced in the distillation, & turned of a blue colour: the same phenom. were produced also by spt of nitre & spt of salt: distilled vinegar made an efferv., but did not discharge any fumes; nor did the liquor turn blue, but of a pale madeira colour.

It appears from hence, that the nitrous acid is so much altered by this process as to have a less affinity to f. alk. than the marine acid, though not so small, I suppose, as distilled vinegar.

Experiments on the solutions of arsenic in the mineral acids—of the arsenical acid—& conjectures concerning its nature.

P. 9. 1·0·0 of arsen. was put into a flor. flask with 2·0·0 of oil of vitr. With the assistance of a heat almost enough to make the oil vitr. boil, it dissolved, but without the least efferv. On cooling, there formed an irregular crystallized mass at bottom, which weighed about 1·10·0; some of this crystallized mass was put into an open cylindrical glass & held over the fire: after some of the moisture had boiled away it dissolved into a transparent glass: after standing some time, it grew opake, & attracted the moisture of the air enough to swell, & burst the glass, but not enough to deliquiate or grow moist. It appears from hence, that arsen. united to the concentrated acid of vitr. bears a much greater heat than either of the 2 substances separate: the same thing is observed of turbeth mineral.

19·4 of arsen. was put into a flor. flask with 2·17·4 of strong spt of salt & about $\frac{1}{3}$ part as much water; it made no efferv. during heating; but by the time the liquor began to boil, it was found to be entirely dissolved: on cooling, a good deal of white matter stuck to the sides, & some small crystals shot.

If some f. alk. is dropt into this solution, it effervesces, & instantly precipitates the arsen. in white clouds.

P. 10. The phenom. attending the solution of arsen. in aqua fortis were found to be very different; for whereas arsen. dissolves very readily in the vitr. & marine acids with the assistance of a sufficient heat, but without the least efferv.: on the contrary, it dissolved very slowly in the nitrous acid, but made a great efferv., & discharged a great quantity of red fumes resembling those produced in the distillation of neut. arsen. salt, & the solution became of a bluish-green; whereas the solut. of arsen. in the vitr. & marine acids did not in the least incline to that colour: moreover the arsen. by being dissolved in this acid was found to have undergone the change necessary to enable it to form the neut. arsen. salt when united to f. alk.: for no arsen. was precip.

from the solut. on saturating it with f. alk., & the saturated solution yielded on evaporation crystals of nitre, mixed with other crystals of a different shape, which proved to be neut. arsen. salt. The success of this exp. induced me to try whether by dissolving arsen. in aq. fort. & driving off the acid by heat I could not procure the arsen. which had suffered the above-mentioned change (or the arsenical acid, if you will allow me to call it by that name) by itself; the success of which was as follows:

1·0·0 of arsen. was put into a flor. flask with 4·15·15 of purified aq. fort., another flor. fl. with a hole in the bottom being inverted into the neck of the 1st, & luted to it: with the assistance of heat it efferved & discharged fumes, which filled both flasks & in part escaped at the hole at top; part of the fumes seemed to condense in the upper flask, & run down in the form of a greenish liquor; after having been kept over the lamp some time it was taken off: the arsen. was not near dissolved; the liquor whilst hot was of a reddish-yellow, like the fumes which were discharged from it, but when cold changed to a deep bluish-green, but which went entirely off in 2 or 3 days: in all probability it proceeded at 1st only from some of the red fumes which were condensed in the flasks & fell down into the liquor: the remainder of this ounce of arsen., & also 3 oz. more were afterwards dissolved in it: the aq. fort. seemed as if it wd have dissolved still more: the solut. was quite clear and transparent, & did not let fall the least sediment as is usual in solutions of metallic substances in this acid: the mixture was found to have lost 16·0 by evap. during this process; this solut. was put into a retort & distilled to dryness in a sand heat; it did not require much heat to do it: at the end of the operation almost as great a heat was given it as the furnace would admit of: no arsen. sublimed; no red fumes nor volatile vapour rose during the distillation; some of the distilled liquor was saturated with f. alk., it seemed to contain little or no arsen., as it made no sensible precip. with blue vitr. or solution of silver or mercury.

P. 12. The caput mortuum remaining in the retort after the distillation weighed 4·13·6, *id est*, about $\frac{1}{6}$ part more than arsen., from which it was made: it attracted the moisture of the air, though but slowly; it requires very little water to dissolve it, I believe scarcely more than $\frac{1}{2}$ its weight; but it does not dissolve fast without the assistance of heat.

Into a solut. of this cap. mort. was dropt some f. alk.; it made a strong efferv.; more f. alk. was dropt in till the efferv. was almost, but not quite, ceased: on evap. it furnished crystals which differed in no respect from the neut. arsen. salt made in the common manner.

The following experiment was made to see whether this caput mort. contained any of the nitrous acid used in the making of it.

The arsen. acid, as I found by an exper. which will be mentioned afterwards, when thoroughly saturated with a calcarious earth, forms a substance which is insoluble in water, & which I beg leave to call calcarious arsenical salt: some of this caput mortuum therefore was dissolved in a good deal of water & saturated with whiting: it made a great efferv., & the calc. arsen. salt fell in flakes to the bottom: after having stood some time, the clear liquor was strained from the insoluble part: this liquor it is plain must con-

tain all the nitrous acid (if there was any in the cap. mort.) under the form of calcarious nitre, but not much of the calc. arsen. salt; but in order to free the calc. nitre more effectually from a little of the calc. arsen. salt, which still remained suspended in the water, the liquor was evap. to dryness, & the solid contents washed with water in order to dissolve all that was soluble: these washings were found by the addition of a little f. alk. and evaporation to contain very little, if any, nitrous salt.

It appears from this experiment that this cap. mort. is the pure arsen. acid, (or the substance, which when united to f. alk. forms the neut. arsen. salt), without any sensible mixture of the nitrous acid.*

It also seems to possess all the properties of an acid (unless perhaps it should fail in respect to taste, which I have not thought proper to try), since it effervesces with & neutralizes the fixed & volatile alcalies, & calcarious earths & magnesia, turns syrup of violets red, & also unites to the earth of alum, which last the sedative salt & sulphur (substances which possess some of the properties of acids, but not all) are not able to do.

The excess of the weight of the cap. mort. above that of the arsen. it was made from must be owing, I suppose, to its retaining some of the water of the aq. fort. used in making it; for the following exper. shews that there is none of the nitrous acid enters into the composition of the arsen. acid, as it shews a way of making the neut. arsen. salt without any thing which contains the nitrous acid.

3·0·0 of arsen. was mixed with 3·1·13 of pearl ashes dissolved in water, so that there was, as well as I can guess, about $\frac{1}{10}$ part more of alk. in proportion to the arsen. than in the neut. arsen. salt: this was boiled till the arsen. dissolved, & then evap. to dryness: some of this mixture was pounded fine & calcined over charcoal in a broad shallow earthen pan, care being taken to keep it frequently stirred: the heat was as great as the matter could bear without caking together. Some of it was taken out now & then, & dissolved in water with solut. silver: the colour of the precipitate formed thereby changed gradually, the more the matter was calcined, from a pale yellow, which it was of at 1st, to a purplish-red, the same as that made by neut. arsen. salt: it was then taken off the fire & dissolved in water: as the f. alk. bore too great a proportion to the arsen. some spt of salt was dropt into it, till it began to efferv. with f. alk.: soon after the spt of salt had been added, it grew muddy, & a small quantity of white sediment fell to the bottom, which seemed to be arsen.; it was then evap.: there 1st shot some crystals resembling neut. arsen. salt, & afterwards some crystals of sal. Sylvii: some of the crystals resembling neut. arsen. salt were dissolved in water; the solut. efferv'd with whiting & f. alk., reddend the colour of blue paper, made the same coloured precip. with solut. silver, & blue vitr., as the neut. arsen. salt; in a word, I could perceive no difference between that & the neut. arsen. salt made in the common manner.

P. 16. I think these experiments shew pretty plainly that the only difference between plain arsenic & the arsen. acid is that the latter is more thoroughly deprived of its phlogiston than the former: for all the ways I know of making arsen. acid or neut. arsen. salt are such as may reasonably

be supposed to deprive the arsen. of its phlogist.: as for example, in making arsen. acid by solution in aq. fort., the nitrous acid is known to have a great disposition to lay hold of phlogiston, & there are strong reasons for thinking that the dissolving of metallic substances in that acid is a very powerful method of depriving them of it, as I shall take notice of by and by. It seems likely too, that the nitrous acid may have the same effect in calcining arsen. & nitre together as in the common way of making neut. arsen. salt; but the above-mentioned way of making neut. arsen. salt by calcining the simple combination of arsen. and f. alk. is a more especial example hereof, since the natural effect of exposing metallic substances at the same time to heat & the open air is to deprive them of their phlogiston: the last exper. too shews that the presence of the open air is almost, if not quite, necessary to produce this change, since the mixture seems in that exper. to have suffered but a small part of the change necessary to turn it into neut. arsen. salt, though exposed both to a greater heat, & for a longer time, than in the former exper.: perhaps too, if the vessel had been more perfectly closed, it wd have suffered still less change.

P. 17. The nature of the difference between the arsen. acid & plain arsen. in some measure favours this opinion, since arsen. acid differs from plain arsen. much in the same manner as that does from the regulus of arsenic; for the regulus of arsen. is indissoluble in water, & has no affinity to f. alk.; white arsenic is in some measure dissoluble in water, & has a very evident affinity to f. alk., thereby manifesting something of an acid property: the arsen. acid is much more dissoluble in water than white arsen., has a strong affinity to f. alk., & seems in all respects a real acid.

If these arguments should seem too hypothetical, the following will most likely be allowed to be satisfactory, namely, that the arsen. acid is easily reduced into regulus by subliming it with inflammable substances. A small quantity of arsen. acid was put into an apothecary's vial with about $\frac{1}{7}$ its weight of linseed oil; it grew soft, mixed uniformly with the oil, & sublimed in the form of regulus, with a less heat than sufficient to make the glass red hot.

The red fumes which issue in the distillation of the neut. arsen. salt & in the dissolution of arsen. in aq. fort. (& consequently the blue aqua fortis, which is only these fumes condensed), can be nothing else, I imagine, than the nitrous acid combined with & volatilized by the phlogiston of the arsen., though I am quite ignorant why they should differ so much both in colour & their greater degree of volatility from the same acid impregnated with phlogiston by dissolving other metallic substances in it. As it appears from a former experiment that little or no arsen. is elevated in drawing off the nitrous acid from a solution of arsenic in aq. fort., & as the arsen. acid made by that means so much exceeds in weight the arsen. it was made from, it does not seem likely that these fumes contain any arsen.

P. 19. Though what I am going to say has not much relation to the present subject, I will beg leave to offer a few conjectures concerning the solution of metals in acids. It is remarkable, that though in general the nitrous acid has the least affinity to metallic substances of any acid, yet it dissolves them with the greatest ease of any: this has been with great reason

attributed to the great affinity of the nitrous acid to phlogiston, part of the acid laying hold of the phlogis. of the metals, & thereby preparing them for dissolution, whilst the remainder dissolves them. In general the nitrous acid dissolves metals with great effervescence, produces a considerable heat, & the vapours produced thereby are of a deeper colour, more pungent, & more elastic, than those of the simple nitrous acid, or than those produced by dissolving alcalies and earths in it, which I think can be owing only to their union with the phlogiston.

As the precipitates from the solution of mercury & the perfect metals in acids are reducible without the help of inflammable matters, it has been thought that those metallic substances are not deprived of their phlogiston by acids; but yet the vol. sulphureous acid produced in dissolving mercury in oil of vitr. seems a strong proof that mercury is deprived of its phlogiston by solution in that acid; & yet (*as you tell me you have tried yourself*) the mercury may be revived from thence without the addition of any matter usually called inflammable; I have found too that the same vol. sulph. acid is produced by dissolving silver in concentrated oil of vitriol: I should imagine therefore that mercury and the perfect metals were deprived of their phlogiston by solution in acids, as well as the imperfect ones, but that by reason of their great affinity to phlogiston they acquired it again from the matter which must be added to separate the acid from them, since there seems no reason to think that the purest f. alk., or even lime, is intirely free from phlogiston. The effervescence & elastic vapours produced during their solution in aq. fort. or aq. regia, seemingly much of the same nature as those attending the solution of the imperfect metals in these acids, very well agree with this hypoth. Whereas it seems likely that if they were not deprived of their phlogiston thereby, they would dissolve quietly without efferv., as arsen. does in the vitr. & marine acids. If this hypoth. is true, it may serve to account for gold not being soluble in any simple acid, but only in aq. regia. Gold, I imagine, has little or no affinity to the nitrous acid, but only to spt of salt; but its affinity to that acid alone is not sufficient to deprive it of its phlogist.: it therefore requires the united efforts of the nitrous & marine acids, the nitrous to absorb the phlogist., & the marine to dissolve the metal. That gold has little or no affinity to the nitrous acid, seems likely, from what Dr. Lewis says,—that gold when by particular management made to dissolve in the nitrous acid is precipitated again only by exposure to the air, & that upon committing a solution of gold in aqua regia to distillation, the nitrous acid flies off leaving the gold united to the spt of salt.

Miscellaneous Experiments on the Arsen. Acid.

P. 22. 3·4 of arsen. acid was put in a small vial covered & intirely immersed in sand in a crucible, so as to shut off all communication with the air: it was calcined in this manner for a good while with a heat raised high enough to make the glass red hot & soft, as appeared from its having received the impression of the sand: no arsenical fumes were perceived; the arsen. acid was not melted, & lost but 6 gra. of its weight, which very likely were only water.

Some more arsen. acid was calcined for a good while in a coffee cup covered in the same manner, with a heat, I should imagine, sufficient to melt copper; no arsenical fumes were perceived on heating, but were very visible when the crucible was taken out of the furnace; almost all the arsen. acid was sublimed: what remained seemed to have been melted, but had no appearance of vitrification.

Some of the arsen. acid was saturated with magnesia: the solution was evaporated, but it did not seem disposed to crystallize: it presented nearly the same phenom. with metallic solutions as the neut. arsen. salt.

Some earth of alum was dissolved in the arsen. acid: it dissolved without efferv., as it does in other acids; it did not seem disposed to crystallize; this solution made a pretty bright scarlet precip. with solut. silver, whereas the neut. arsen. salt makes a sort of purplish-red: the phenom. which it presents with other metallic soluts are not remarkably different from those made by the neut. arsen. salt.

The arsen. acid itself makes much the same coloured precipitates with solut. silver & mercury in nitrous acid, & of tin in spt of salt, as the neut. arsen. salt: with most other metallic solutions it makes no precip.

Neither the neut. arsen. salt nor any other combination of the arsen. acid makes any precip. with solut. nickel in aq. regia, and but very little with the red tincture extracted from zaffer by aq. regia, *id est*, a solution of regulus of cobalt: even that little seems owing to the bismuth contained in it.

EXPERIMENTS ON FACTITIOUS AIR.

[The date of these experiments is probably not later than 1767.]

PART IV.

Containing experiments on the air produced from vegetable and animal substances by distillation.

I received the air produced from these substances in inverted bottles of water, nearly in the same manner as in the former experiments read to this Society, by means of the apparatus represented in the annexed drawing.

Exp. 1. 400 grains of raspings of Norway oak, called wainscot by the carpenters, were distilled in the above-mentioned manner, till no more air would rise with a heat just sufficient to make the distilling vessel obscurely red hot. The bottle in which the air was received was then removed, & another put in its place, & the distillation completed with a pretty strong red heat. By this means that part of the air which requires a red heat to disengage it was procured separate from that which rises with a less heat. Each of these parcels of air were then brought in contact with sope leys in the manner described in my experiments on Rathbone-place water, in order to see whether they contained any fixed airs, & to free them from it if there was any. The first parcel of air, namely that which rose first in distillation, measured 22100 grains when first made, & was reduced by the sope leys to 12700.

The second parcel measured 34600 grains, & was reduced by the same means to 30700 grains.

The quantity of common air contained in the distilling apparatus, allowing for the room occupied by the wood was about 1700 grains; all of which must have been forced into the inverted bottle along with the first distilled parcel of air and would not be absorbed by the sope leys. 1700 is about $\frac{2}{15}$ of 12700; so that the first distilled air when reduced by the sope leys, contains about $\frac{2}{15}$ of its bulk of common air, or is a mixture of about 13 parts of pure factitious air to 2 of common air. The last distilled parcel must have been intirely free from common air.

All that air which was absorbed by the sope leys may, I think, be fairly supposed to be fixed air. The remaining air of each parcel was inflammable, but required a much greater quantity of common air to make it explode than the inflammable air from metals does: for a vial holding near 1200 grains measure being filled with 1 part of the first distilled air with $2\frac{1}{2}$ of common air, the moisture caught fire on applying a lighted candle to the mouth of the vial & went off with a small puff; but when the vial was filled with 1 part of the same air to 2 of common air it would not catch fire. In like manner a mixture of 1 part of the 2nd distilled air with 3 of common air went off with a puff, but 1 part of the same air with $2\frac{1}{2}$ of common air would not. So that the first distilled air required to be mixed with not less than between 2 and $2\frac{1}{2}$ times its bulk of common air, & the 2nd distilled air with between $2\frac{1}{2}$ and 3 times its bulk of common air, before it would explode; whereas the air from metals, when tried the same way, would explode though mixed with only $\frac{1}{2}$ its bulk of common air.

I next tried which of these parcels of air would explode with most force when mixed with considerably more common air than what was sufficient to enable them to catch fire. For this purpose I mixed some of each of these parcels of air & also some inflammable air from zinc with 4 times their bulk of common air, & tried them in the same bottle. The first distilled air went off with the least noise. As for the 2 others, I was uncertain which made most; but the air from zinc went off with a sharper sound than the other, & no light could be seen in the bottle; whereas in the trial of each of the distilled airs a small light was seen.

The experiment was then repeated with mixtures of each of these airs with 5 times their bulk of common air. The first distilled air took fire, but scarce any noise. The 2 others went off as near as I could judge with the same degree of noise, the distilled air with a small light visible in the bottle & a duller sound, the air from zinc without any light & a sharper sound.

It should seem therefore as if the 2nd distilled air contained about as much phlogiston as the air from zinc, but that the first did not contain so much: for when the quantity of common air is considerably more than sufficient to consume the whole of the inflammable air, it seems likely that the loudness of the explosion should be in proportion to the quantity of phlogiston contained in the mixture.

In all these experiments the air was measured in a cylindrical glass with divisions on its sides, in such manner that I think I could not well err more

than 5 grains or a 240th part of the whole mixture. The vial in which the explosion was made had a glass tube about an inch & $\frac{1}{2}$ long & $\frac{4}{10}$ of an inch in bore fitted to its mouth, by way of contracting the orifice.

I also tried the specific gravity of each of these parcels of distilled air in my usual manner. 10,000 grains of the first distilled air being forced into a bladder, which held 48,000 grains & had a brass cock fitted to it, the bladder increased $\frac{3}{4}$ of a grain in weight on pressing out the air. So that, supposing common air to be 800 times lighter than water, this air which was before said to contain $\frac{2}{15}$ of its bulk of common air should be about $\frac{1}{17}$th part lighter than common air; & the pure factitious air without any mixture of common air should be $\frac{1}{14}$th or $\frac{1}{15}$th part lighter than common air, or near $6\frac{1}{2}$ times heavier than inflammable air from metals.

21100 grain measures of the last distilled air being forced into the same bladder, there was an increase of 12 grains on pressing it out; whence this air appears to be lighter than common air in the proportion of 11 to 6, or near 4 times heavier than the air from metals.

The caput mortuum or matter remaining in the brass pot after the distillation was completed, consisting of the wood reduced to charcoal, weighed 134 grains.

On the whole, the 400 grains of wainscot yielded with a heat less than sufficient to make it red hot 9400 gra. measures of fixed air, whose specific gravity was before found to be about $1\frac{1}{2}$ times greater than that of common air, & 12700 of an inflammable air, which was about $\frac{1}{15}$ parts lighter than common air, & which required to be mixed with more than 2ce its bulk of common air to make it explode. With a greater heat than that it yielded 5800 grains of fixed air, & 30700 of an inflammable air, which required to be mixed with above $2\frac{1}{2}$ times its bulk of common air to make it explode, & whose density was $\frac{6}{11}$ of that of common air. The weight of all this air together is 64 grains, *id est*, $\frac{16}{100}$ of the weight of the wood it was produced from, or near $\frac{1}{4}$ of the loss of weight which it suffered in distillation. It must however be observed that there was most likely more fixed air discharged than is here set down; as in all probability some of it must have been absorbed by the water.

As this inflammable distilled air is much heavier than that from metals, & requires to be mixed with a much greater proportion of common air to make it explode, I at first imagined it might consist of an inflammable air exactly of the same kind as that from metals, mixed with a good deal of air, heavier than it, & which had a power of extinguishing flame like fixed air; as I hinted before with regard to the air produced from meat by putrefaction: but on consideration, I fancy this must really be of a different kind from that of metals; for if it had been only a compound of that air with some of a different kind, then a mixture of that compound with common air must necessarily, I think, have exploded with less noise than a mixture of pure inflammable air with the same proportion of common air; as it contains less inflammable matter than the latter mixture, & that compounded with a substance which should rather diminish than increase the explosion; whereas the last distilled air was found to make as great an explosion as the air from metals, when both were mixed with 4 times their bulk of common air.

Exp. 2. In another trial made in the same manner, except that the whole of the distilled air was received together, without changing the bottle, the like quantity of wainscot yielded 19200 grain measures of fixed & 42700 of inflammable air. The inflammable air requires to be mixed with more than 2ce its bulk of common air to make it explode, & its density was less than that of common air in the proportion of 1·52 to 1. The weight of the whole of this air is 71 grains, *id est*, near $\frac{18}{100}$ of the wainscot it was produced from. This experiment is exactly consistent with the former, except that the quantity of fixed air was greater, as might be expected, since the distillation was performed in much less time, & consequently much less fixed air could be absorbed by the water.

Exp. 3. I made another experiment with the same quantity of wainscot, the distilling pot being this time placed in oil, that I might see what would be the nature of the air which would rise with no greater heat than that of boiling oil. The oil caught fire, which prevented me from completing the experiment; I however got 11500 grain measures of air, 5400 of which were fixed air, the remaining 6100 were inflammable, requiring somewhat more than 2ce their bulk of common air to make them explode. Their density, allowing for the common air in the distilling vessel, was about $\frac{1}{18}$ part greater than that of common air.

Exp. 4. I also examined the air produced from tartar by distillation, though not in so careful a manner as the wainscot. It yielded more fixed & less inflammable air than wainscot; 400 grains of it yielded 46600 grains of fixed air & 23500 of inflammable air. The inflammable air required to be mixed with more than 4 times its bulk of common air to make it explode, & was about $\frac{1}{11}$ part heavier than common air.

Exp. 5. 900 grains of hartshorn shavings were distilled exactly in the same manner as the wainscot in the first experiment, except that the heat was raised to a rather greater degree before the bottle was changed. The first distilled parcel of air measured 33600 grains, & was reduced by sope leys to 20400. The common air left in the distilling vessel was 1630 grains; so that this air when reduced by the sope leys contained $\frac{4}{50}$ of its bulk of common air. The last distilled parcel measured 9400 grains, & was reduced by sope leys to 8900.

Each of these parcels of air, when thus reduced, was found to be inflammable. The first distilled air, tried in the same bottle as was used for similar experiments on the air from wainscot, caught fire on applying a lighted candle when mixed with 5 times its bulk of common air, but would not when mixed with only 4 times its bulk. The 2nd parcel caught fire when mixed with $2\frac{1}{2}$ times its bulk of common air, but would not with 2ce its bulk. I then compared the loudness of the explosion made by each of these parcels of air & of some air from zinc, when mixed with 6 times their bulk of common air; I could perceive very little difference between the 2 parcels of distilled air, but both of them seemed to make rather more noise than the air from zinc. The same difference in the manner of explosion between the distilled air and air from zinc might be observed with these as with that from wainscot; namely, that the distilled airs went off with the duller sound, & exhibited a light in the bottle, which was not visible with the air from zinc.

18240 grain measures of the first distilled air being forced into a bladder holding about 21600, there was an increase of weight of $5\frac{3}{4}$ grains on pressing out the air; so that, allowing for the common air mixed with it, the pure factitious air is lighter than water in the proportion of 137 to 100.

8160 grain measures of the 2nd distilled air being forced into a bladder holding near 14000, it increased $4\frac{1}{4}$ grains on pressing out the air, whence it appears to be lighter than common air in the proportion of 171 to 100.

The caput mortuum, consisting of the hartshorn burnt to a coal, weighed 623 grains. The weight of all the air discharged appears, from what has been said, to be 51 grains, *id est*, $\frac{1}{16}$ part of the weight of the hartshorn, or about $\frac{2}{11}$ of the loss of weight which it suffered in distillation.

We have examined, therefore, 3 substances of very different natures, namely, the first a simple wood, the 2nd a vegetable substance of a saline nature, & the 3rd an animal substance of the nature of bones. Each of them agreed in furnishing some fixed & some inflammable air, but the proportions of these airs were considerably different, & the nature of the inflammable air was not quite the same in each, but yet hardly differing more than that produced from the same substance at different periods of the distillation; so that there should seem to be a considerable resemblance between the air produced by distillation from all animal & vegetable substances.

In the first & 2nd experiments we have an examination of all the air which can be procured from wainscot by distillation in close vessels; but this is by no means all the air which it contains; for the caput mortuum, which, as was before said, consists of the wood burnt to charcoal, seems to contain a very remarkable quantity of fixed air.

The alcali produced by deflagrating nitre with charcoal is well known to effervesce with acids, & consequently to contain fixed air; which air, I think, can proceed only from the charcoal; for when nitre is alcalized by metals in their metallic form, which contain no fixed air, the alcali makes no effervescence with acids; as I know by experience: & I think it seems very unlikely that the nitre should furnish fixed air when deflagrated by charcoal, & not produce any when deflagrated by metals. This induced me to make the following experiments.

Exp. 6. 150 grains of the caput mortuum remaining after the distillation of wainscot in the first & 2nd experiments, well dried, were ground with 5 times their weight of nitre and about 130 grains of water, & when the whole was thought to be perfectly mixed, it was deflagrated by little & little in an iron ladle. The intention of the water was to make the matter deflagrate with less violence; whereby there was less danger of any fixed air being dissipated by the heat. The deflagrated matter was put into water to dissolve the alcali. The insoluble matter, consisting partly of the ashes of the caput mortuum & partly of some of the caput mortuum which had escaped the fire, weighed, when well dried, 38 grains; so that the loss of weight which the caput mortuum suffered in deflagration was 112 grains. In order to find the quantity of fixed air in the alcaline solution, $\frac{1}{2}$ of it was saturated with the vitriolic acid, & the loss of weight which it suffered in effervescence observed with the same precautions as were used for finding the quantity of fixed air in pearl

ashes in the 2nd part of these experiments: it appeared to contain 62 grains. As this experiment makes the quantity of fixed air produced from the caput mortuum appear to be greater than the loss of weight which it suffered in deflagration, which is impossible, I took another method to find the quantity in the remaining ½ of the alcaline solution; namely, I mixed it with a sufficient quantity of lime water, whereby all the fixed air therein was transferd into the lime, which was thereby precipitated.

I then found the quantity of fixed air in this precipitate; it appeared to be 59 grains, which is only 3 grains less than it appeard to be the other way. By a mean of these experiments, the quantity of fixed air separated from the 150 grains of caput mortuum should be 121 grains, which is 9 grains more than the loss of weight which it suffered in deflagration.

By a like experiment made with some more caput mortuum of the same kind the quantity of fixed air seemd still greater.

As it is impossible that the quantity of fixed air separated from the caput mortuum should exceed the loss of weight which it suffers in deflagration, I must either be mistaken in supposing that all the fixed air in the alcali proceeded from the caput mortuum, & not from the nitre; or else some moisture must have flown off along with the fixed air in saturating the alcali with the acid: which would make the quantity of fixed air therein appear greater than it really is. This last supposition seems much the most probable.

PAPER ON MEPHITIC AIRS.

[On this Paper Cavendish has written, "communicated to Dr. Priestley." In the account given by Priestley of his *Experiments and Observations made in and before the year* 1772 (Sect. 6. Ed. 1774. p. 109.), he says, "Ever since I first read Dr. Hales's most excellent Statistical Essays, I was particularly struck with that experiment of his, in which common air and air generated from the Walton pyrites by spirit of nitre, made a turbid red mixture, and in which part of the common air was absorbed; but I never expected to have the satisfaction of seeing this remarkable appearance, supposing it to be peculiar to that particular. Happening to mention this subject to Mr. Cavendish, when I was in London in the spring of the year 1772, he said that he did not imagine but that other kinds of pyrites, or the metals, might answer as well, and that probably the red appearance of the mixture depended upon the spirit of nitre only: this encouraged me to attend to the subject." We have already seen the notice which Cavendish had taken of nitrous gas and nitrous acid as early as 1764. Section 9, p. 143, begins thus:—"Being very much struck with the result of an experiment of the Hon. Mr. Cavendish, related Phil. Trans. vol. lvi. by which, though he says he was not able to get any inflammable air from copper by means of spirit of salt, he got a much more remarkable kind of air, one that lost its elasticity by coming into contact with water, I was exceedingly desirous of making myself acquainted with it." In Section 7 of the same account of his Experiments in and before 1772 (*Ibid.* p. 129), Priestley adds, "Air infected with the fumes of burning charcoal is well known to be noxi-

ous, and the Hon. Mr. Cavendish favoured me with an account of some experiments of his, in which a quantity of common air was reduced from 180 to 162 ounce measures, by passing through a red hot iron tube filled with the dust of charcoal. This diminution he ascribed to such a *destruction* of common air as Dr. Hales imagined to be the consequence of burning. Mr. Cavendish also observed that there had been a generation of fixed air in this process, but that it was absorbed by soap leys. This experiment I also repeated, with a small variation of circumstances, and with the same result." The following paper, containing the first clear description of nitrogen as a distinct gas, is the communication thus defectively described. Cavendish, in fact, was the first to point out, as distinct *factitious airs*, besides hydrogen, the carburetted gases, nitrous gas, muriatic acid gas, and nitrogen]:—

Paper communicated to Dr. Priestley.

The receiver used in the 9th experiment of my 2nd paper on factitious air was a bolthead, from which I had cut off the greatest part of the neck, & thereby consisted of a globular body about 9 inches in diameter, with a neck about 2 or 3 inches in diameter & about 2 inches long.

As the fixed air was let into the receiver first, & the common air afterwards, I think they could hardly fail of being well mixed together by the commotion made by letting in the common air. However, as Dr. Priestley thinks they were not, & that it was owing to that that the candle went out so soon, I made an experiment to see whether they were well mixed or not; this I did by seeing whether the candle would burn as long in a mixture containing $\frac{10}{112}$ of fixed air, when held near the bottom of the receiver, as when held near the top. For if the air was not perfectly mixed, the fixed air, as being the heaviest, would have kept chiefly at the bottom of the receiver, & consequently the candle would have burnt longer at the top than at the bottom of it. The event was as follows:—When the candle was held at the bottom of the receiver it burnt 21″, when held near the top it burnt in three different trials 17″, 26″, & 19″. The same candle burnt, in the same receiver filled with common air only, when held near the bottom, 82″ & 69″; & when held near the top it burnt 79″ and 66″; so that the 2 sorts of air seem to have been perfectly mixed. The experiment was tried just in the same manner & with the same receiver as that related in the Transactions; & the candle, in those trials where I have said it was held near the bottom of the receiver, was held in the same situation as in that experiment. N.B. It was by mistake that I made only 1 trial with the candle near the bottom & 3 with it near the top, as I intended to make two trials in each manner.

There is no experiment related in that letter of Dr. Priestley's which you read to me, that shows that mephitic air will mix with common air in time, of itself, without any shaking, as he says that phials of common air, held in vessels of mephitic air, became mephitic, which could only be owing to some of the mephitic air mixing with the common air therein, which, according to my experiment, wd render it unfit for candles to burn in, & in all probability wd render it unfit for breathing, which is what I suppose he means by becoming mephitic. I am not certain what it is which Dr. P. means by mephitic air, though from some circumstances I guess that what he

speaks of in this letter was that to which Dr. Black has given the name of fixed air. The natural meaning of mephitic air is any air which suffocates animals (& this is what Dr. Priestley seems to mean by the word), but in all probability there are many kinds of air which possess this property. I am sure there are 2, namely, fixed air, & common air in which candles have burnt, or which has passed thro' the fire. Air which has passed thro' a charcoal fire contains a great deal of fixed air, which is generated from the charcoal, but it consists principally of common air, which has suffered a change in its nature from the fire. As I formerly made an experiment on this subject, which seems to contain some new circumstances, I will here set it down.

I transferd some common air out of one receiver through burning charcoal into a 2nd receiver by means of a bent pipe, the middle of which was filled with powdered charcoal & heated red hot, both receivers being inverted into vessels of water, & the 2nd receiver being full of water, so that no air could get into it but what came out of the first receiver & passed through the charcoal. The quant. air driven out of the first receiver was 180 oz. measures, that driven into the 2nd receiver was 190 oz. measures. In order to see whether any of this was fixed air, some sope leys was mixed with the water in the bason, into which the mouth of this 2nd receiver was immersed; it was thereby reduced to 168 oz., so that 24 oz. meas. were absorbed by the sope leys, all of which we may conclude to be fixed air produced from the charcoal; therefore 14 oz. of common air were absorbed by the fumes of the burning charcoal, agreeable to what Dr. Hales and others have observed, that all burning bodies absorb air. The 166 oz. of air remaining were passed back again in the same manner as before, through fresh burning charcoal into another receiver; it then measured 167 oz., & was reduced by sope leys to 162 oz., so that this time only 5 oz. of fixed air were gen. from the charcoal, & only 4 oz. of common air absorbed. The reason of this is, that since the air was rendered almost unfit for making bodies burn by passing once through the charcoal, not much charcoal could be consumed by it the 2nd time; for charcoal will not burn without the assistance of fresh air, & consequently not much fixed air could be generated, nor much common air absorbed. The specific gravity of this air was found to differ very little from that of common air; of the two it seemed rather lighter. It extinguished flame, & rendered common air unfit for making bodies burn, in the same manner as fixed air, but in a less degree, as a candle which burnt about 80″ in pure common air, & which went out immediately in common air mixed with $\frac{6}{55}$ of fixed air, burnt about 26″ in common air mixed with the same portion of this burnt air.

LETTER OF CAVENDISH TO MONGEZ.

A Londres, ce 22 Fevrier, 1785.

En lisant, Monsieur, la traduction de mon mémoire sur l'air publié dans le *Journal de Physique*, je fus frappé de le voir datté de Janvier 83, comme si la lecture en eut été faite *alors* devant la Société Royale. J'eus recours aux exemplaires détachés imprimés pour l'usage de mes amis sur l'un desquels

apparemment avoit été faite votre traduction ; je trouvai à mon grand étonnement, que l'imprimeur avoit fait cette même faute dans toutes les copies, malgré que l'original publié dans les Transactions Philosophiques avoit été datté, comme il devoit l'etre, de Janvier 84. Je vous serai très obligé, Monsieur, de vouloir bien faire mention de cette méprise dans le cahier prochain de votre Journal.

Je suis mortifié d'etre dans le cas d'ajouter qu'il s'en faut de beaucoup que la traduction soit exacte ; on a manqué le sens en plusieurs endroits.

J'ai l'honneur d'etre avec des sentimens distingués,

Monsieur,

Votre très humble et très obeiss[t] serviteur.

A Monsieur T. A. Mongez, le Jeune, &c. &c. &c.,
Au Bureau du Journal de Physique à Paris.

EXTRACT FROM A LETTER OF CAVENDISH TO DR. BLAGDEN.

I have been reading La V. preface. It has only served the more to convince me of the impropriety of systematic names in chemistry, & the great mischief which will follow from his scheme, if it should come into use. He says, very justly, that the only way to avoid false opinions is to suppress reasoning as much as possible, unless of the most simple kind, & reduce it perpetually to the test of experiment; & can anything tend more to rivet a theory in the minds of learners than to found all the names which they are to use upon that theory?

But the great inconvenience, is the confusion which will arise from the different hypotheses entertained by different people, & the different notions which must be expected to arise from the improvements continually making. If the giving systematic names becomes the fashion, it must be expected that other chemists, who differ from these in theory, will give other names agreeing with their particular theories, so that we shall have as many different sets of names as there are theories: in order to understand the meaning of the names a person employs, it will be necessary first to inform yourself what theories he adopts. An equal inconvenience, too, will arise from the necessity of altering the terms as often as new experiments point out inaccuracies in our notions, or give us further knowledge of the composition of bodies. But to show the ill consequence of what they are about, let them only consider what would be the present confusion, if it had formerly been the fashion to give systematic names, & that those names had been continually altered as people's opinions altered. The great inconvenience is the fashion which so much prevails among philosophers, of giving new names whenever they think the old ones improper, as they call it. If a name is in use, & its meaning well ascertained, there is no inconvenience arises from its conveying an improper idea of the nature of the thing; & the attempting to alter it serves only to make it more difficult to understand people's meaning.

With regard to distinguishing the neutral salts of less common use by names expressive of the substances they are composed of, the case is different; for their number is so great, that it would be endless to attempt to distinguish

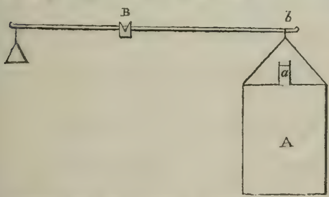
them otherwise; but as to those in common use, or which are found naturally existing, I think it would be better retaining the old names. And with regard to salts whose properties alter according to the manner of preparing them, such as corrosive sublimate, calomel, &c. &c., I should in particular think it very wrong to attempt to give them names expressive of their composition.

As I think this attempt a very mischievous one, it has provoked me to go out of my usual way & give you a long sermon. I do not imagine, indeed, that their nomenclature will ever come into use; but I am much afraid it will do mischief, by setting people's minds afloat, & increasing the present rage of name-making.

EXPERIMENTS ON AIR.

[These experiments occupy about 100 sheets, of 4 pages each, of which some are blank, in whole or in part: they are numbered and indexed, as well as written, by Cavendish's hand: their arrangement is generally in the order of time, but on making a second experiment to the same effect as one made before, he has sometimes entered it out of that order, on a blank page, after the first: thus at page 128 an experiment of Nov. 1782 is inserted among those made in 1781, of which it was a repetition. The following lithographic extracts contain all his experiments relating directly to the composition of water, & involve all the reasonings of his paper in the Transactions on this subject: the remainder relate chiefly to the analysis of air, and of the gases proceeding from charcoal, and from its combustion with nitre. Among those which he did not think it worth while to relate, is one which shows the unsettled state of opinion respecting the general properties of gases.]

Note-book, p. 80.—"It was tried whether the *vis inertiæ* of phlogisticated was the same in proportion to its weight as that of common air, by finding the time in which a given quantity passed through a given hole, when urged by a given pressure, by means of the following apparatus:—



"A is a tin vessel, eight and a half inches in diameter and ten deep, with a small hole in the diaphragm, *a*. This vessel is suspended over a vessel of water by the rod B, turning on a centre near the middle point, and partly balanced by a weight at the other end, and suffered to descend as the air runs through the hole. The time in which it descended a given space (about seven inches and a half), was found by observing the time in which the knob *b* moved from one mark to another. The force with which the vessel was pressed down was about ten and a half ounces, the rest of the weight of the tin vessel being taken off by the counterpoise. The way by which it was filled with air was, by holding it under water till all the air

was run out, then stopping the pipe with the thumb, raising up the vessel till the bottom was near the surface, and pouring in the air. The event was as follows, October 28, 1780:

With common air it was .... 2·15 running out.
A second time .... 2·12½
With air phlog. by liv. sulpht. 2·7
With com. air .... 2·9

"The spe. gra. of this air was tried by forcing 13895 of it into a bladder, when it increased $\frac{4}{10}$ of gra., by forcing out the air: therefore spe. gra. $= \frac{1}{47}$ less than that of com. air: its test

With N. air was.... 186
Com. air 197
Two measures of air 201·8."

P. 115

Explosion of inflam. air by el. in glass globe to examine Mr Warltires experiment

The globe holds 24421 the cap to D° 284 in all 24705

July 3

14200 of inflam. air from zinc was put into this globe the rest being com. air It would not take fire About 900 of this air including that in cap was then let out & its place supplied by com. air conseq. the inflam. air remaining = 13680 or .554 of the whole It caught fire & the glass seemed rather warmer than before but there was no other signs of it Its weigt was not at all alterd ($\frac{1}{5}$ of gra. would have been percept. with tolerable certainty) on opening it under water it was found that 7564 or .305 of the whole was absorbed its test tried July 8 was .055 with dist. water *

July 4 4220 of inf. air or ,1708 P.116

$\frac{1}{5,9}$ of whole contents was put into this bott It seemed to lose $\frac{1}{10}$ gra on firing & was warmer than in former & on remaining about $\frac{1}{2}$ hour more seemed to lose about $\frac{1}{10}$ more it then felt quite cool on opening it under water 5790 run in & after shaking it about a good deal 224 more so that the whole quant. absorbed was 6014 or $\frac{1}{4,106}$ of the whole

Its test tried July 8 was ,648

Its spe. gra. was $\frac{1}{174}$ less than com. air as 13900 being tried in bladder it inc $\frac{1}{10}$ gra on driving it out

---

* prev. P its spe. gra. was less than that of com. air by $\frac{1}{2.8}$ as 13900 gra. being forced into bladder it increased 6.3 gra on driving it out

same day 6140 of inf air or $\frac{1}{4.024}$ of whole contents was put into the bott. it did not lose at all on firing nor on standing $\frac{1}{2}$ hour 8615 of water run in on opening & 230 more on shaking in all 8845 or

Its test was .339

Its spe. gra. was $\frac{1}{50}$ less than com. air 9900 being tried & inc weight $\frac{1}{4}$ gra

---

July 5 7344 of inf air or $\frac{1}{3.36}$ of whole contents was put in it seemed to lose $\frac{1}{10}$ gra on firing & seemed to inc near as much more on standing $\frac{1}{2}$ hour 10490 of water run in on opening & 140 more on shaking in all 10630 Its test was .097

Its spe. gra was $\frac{1}{62}$ lighter than com. air 9900 being tried & inc. weight = $\frac{2}{10}$

July 6 12683 of inf. air or
of whole contents was put into
bott. the rest com. air it seemed
to lose $\frac{1}{5}$ gra. on firing & did not
alter on standing 7720 of water
run in on opening & no more run in
on shaking The test was ,063
Its spe. gra was $\frac{10}{29}$ lighter than
com. air 9900 being tried & inc
weight = 4.2
Same day 10225 of inf. air or
of whole was put into bott.
the rest com. air it seemed to lose
1 or 2 10th gra. on firing It did not
alter on standing 9269 of water
run in on opening & 100 more on shaking
in all 9369 or The test was ,066
Its spe. gra was $\frac{1}{6}$ lighter than
com. air 13900 being tried &
inc. weight being 2.9
All these spe. gra. were tried
July 17

globe holds 24705

Inf. air	13680	4220	6140	7344	12683	10225
~~air abs.~~	~~7564~~	204[illegible]	18565	17361	1202[illegible]	14480
com. air	11025					
air abs	7564	6014	8845	10630	7720	9369
mixt	~~17144~~					
Inf	4.1361	6253	7882	8659	4.1032	4.0097
com	.0424	3114	2687	.2396	.0800	.1608
abs	8788	7792	9467	.0265	8876	.9717
	0937	3139	5195	6.263	0232	8489
	8364	4678	6780	7869	8076	8109
Inf	1.241	.206	.331	.423	1.055	.706
abs	.686	.294	.476	.612	.642	.647
mixt	1.555	.912	.855	.811	1.413	1.059
test	.055	.648	.339	.097	.063	.066

From the 4th mixt it should seem as if .44 of infl. were suff to phlogist 100 of common in which case infl is very little more ~~than~~ 3/10 of whole & the bulk mixt should then be .803 of com. air

·208 of fine unneald iron wire dissolved in mixt 1 meas. oil of vitr. with 2 of water dissolved briskly & produced 2 round botts full & 1 large bott. about $\frac{3}{4}$ or $\frac{4}{5}$ full

Aug. 4

The exper. of P. 115 being repeated with this air the quant. inflam. air being 6030 its weight seemed diminished $\frac{3}{10}$ on firing & increased $\frac{1}{10}$ on standing $\frac{1}{2}$ hour 8435 were found to be absorbed & 100 more on shaking in all 8535 N.B. it was found before letting in the water that the screw was not tight so that ~~it might~~ perhaps ~~have absorbed 1~~ some air might have got in its Test was ·350

Aug. 4 the exper was repeated only exhausting air out of globe & letting it absorb the air ready mixt through bent tube the air consisted of 3 parts com. to one of the abovementiond inflam. air the air was exhausted from globe till standard gage stood at 29.9 bar. being at 30.39 so that quant. air remaining supposing none of it to be vapour should be $\frac{1}{62}$ of whole Its weight seemed not alterd on firing 88 40 were absorbed with shaking its test was ,364

The globe was dry before firing but was immed. coverd. with dew on firing

The cock was found before hand to be suffic. air tight

Aug. 7 The exper was tried with the same air in the 1st manner with 6,35 of inf. air the rest common its weight seemed to be dimin $\frac{2}{10}$ on firing & $\frac{1}{10}$ more on standing 8578 were absorbed & 140 more on shaking in all 8718 its test was ,353

The exper was tried in same manner with the air from zinc used before the inf. air was 6118 the rest com. it was not observed whether weight alterd 8655 were absorbed & 72 more on shaking in all 8727 its test was ,341

(Page 123 is blank, Ed.)

$$\frac{24707}{62} = 399$$

$$\text{inf} = \frac{24707-399}{4} = 6077$$

	air from iron 24705	air from iron	air from zinc	
inf. –	6030	6135	6118	6077 air from iron tried
com –	18675	18570	18587	18628 by air pump
abs –	8535	8718	8727	8840
	7803	7878	7866	7837
	2713	2688	2693	2702
	9312	9404	9409	9465
	9.5090	5190	5173	5135
	6599	6716	6716	6763
inf –	·3228	·3304	·3291	·3262
abs –	·4570	·4695	·4695	·4746
mixt -	·8659	·8609	·8596	·8516

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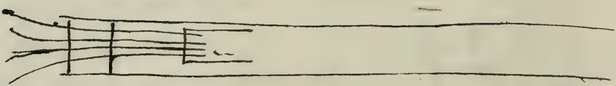
P.126



About 3720 cyl. inc or 744000
gra. meas were burnt with about
2/5 of that quant. of inflam. air
from zinc & the air made to pass
through 2 long glass cylinder together ~~about~~
~~3/4 in diam. & upper~~ upwards
of 10 feet long & about 3/4 inc
diam. the 2 airs were conveyed
into the cylinder by separate copper
pipes opening close together as in
figure the ends of the 2 pipes ~~were~~
also opened into a small short
brass cyl. as in figure in order
to confine the air to be burnt in
smaller space The 2 airs were
contain'd in separate large tin vessels
inverted into tin pots of water & the
airs forced out in proper proport.
by pouring water into those vessels
by 2 holes ~~of~~ ~~prope~~ diff: sizes

in bott. ~~tin pot~~ of the same pot P 127

About 135 of water were caught besides some which was lost by accident The cylinder near that part where the air was fired seemed a little tinged with sooty matter but very slightly & that perhaps might proceed from the putty with which the apparatus was luted The water said to be caught had no tast nor smell it was evap. to dryness in glass cup without leaving any sensible quantity of sediment it gave no pungent smell on evap.

About an hour or 2 after this air was burnt the remainder of the ~~air~~ inflam. air in the tin vessel was attempted to be burnt in the same manner but it did not burn well In order to see whether this was owing to the inflam. air being alterd by the tin vessel 2 botts were filled with inf. air & some bits of tin plate put in one & some iron wire

Sun. July

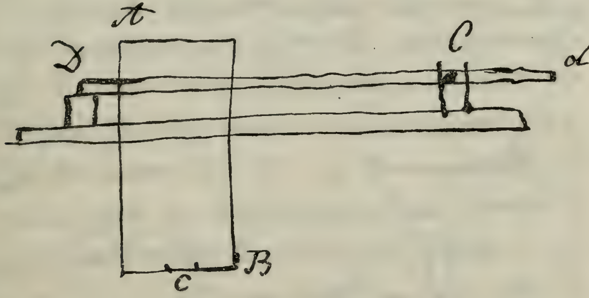
P. 128

in the other These were kept ~~till~~ many days without the air seeming to be sensibly diminished

*Nov. 18 1782 This exper. was repeated the quantity of each air burnt was not well known about 150 grs. of water was caught which was not at all acid nor ~~at~~ gave the least red colour to paper tinged with red flowers it yielded no pungent fumes on evap. & yielded scarce any sediment on evap. to dryness

(*Subsequent insertion on the blank page. — Page 129 is blank, Ed.)

Measure of explosions of inflam.



The strength of the explosion was tried by the ~~fol~~ machine repr in fig. AB is the brass cyl. for the air with a hole c at bottom. This is fixed tight to a board Dd turning on center at C & more or less weight is laid on board Dd ~~as it was supposed that force with which the cyl would be pushed up would be about~~ In using it the cyl was first filled with water ~~the app~~. the water entering by hole at bott. & the air escaping by cock at top the app. for firing the air by el. being placed in recess so as not to be wetted thereby

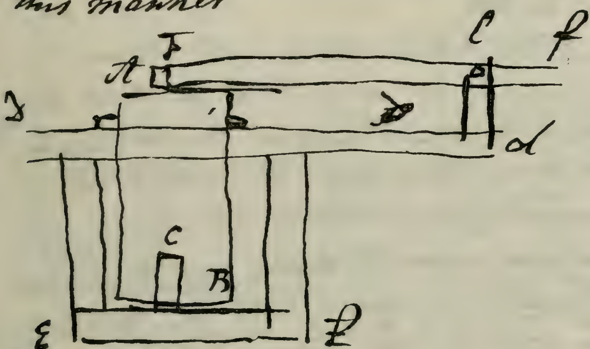
A proper quantity of ~~[illegible]~~ air was then pourd in by hole at bott (but not near suffic. to fill the whole cyl) & fired & the height which the cyl. sprung up thereby measured by an index The force with which the cyl. was pressed down was found by ~~[illegible]~~ it to the end of pair of scales ~~by a w~~ while in its place by a wire fastend to the middle of the top of the cyl.

The hole was exactly $\frac{1}{2}$ inc. in diam. the cyl. was about 6 inc. long & ~~held~~ 1.8 in diam & held. 4300 grs. the recep in which the app. for firing was placed held about 12 grs

1713 of a mixt of 1 part of inf. & 2 of common air being put into this machine ~~&~~ fired it sprung up 4.23 when weight lying on it was 32400 & 3.43 when the weight was 37300 P.131

(Page 132 is blank. Ed.)

As it was suspected that the water driven out from the hole by the explosion was resisted by the water below & thereby was pressed against the bott. of the cyl. & made the force appear too great the machine was altered in this manner



AB is the cylinder passed through a round hole in the fixed board Dd so as to rise & fall in it freely & resting thereon by a projecting piece of brass c is a cylindrical piece of brass filling up the whole almost intirely & fixed upon the piece of wood Ee fastend to the

bottom of the board Dd Ff is a board turning on a center at C & resting on the brass cyl. intended to carry weight mixt air is put into the cyl & fired as before & the height which it rises measured by index as before

As the bottom of the cyl. almost touchd the wood Ee it was suspected that the small quant. water issuing between the plug & the sides of the hole might have partly the same effect as was apprehended in the former machine a small piece of wood was in some trials placed between Ee & the bott. cyl. so as to keep it raised about $\frac{1}{4}$ inch above it but the effect was the contrary to what was expected as it always sprung up more when this wood was placed under than without it some of the obs. made with it are given below

ind before firing	D° after	weights lying on board	Dist nearest end from end board	weight with which cyl is pressed P.135
0.	0.	1+2+3	14.6	~~[illegible]~~
,72	,77	— — —		D°
,0	,0	1+3+4	14.6	84000
,73	,82	— —	— —	D°
,0	,16	1 — —	14.6	68000
,74	1.27	— —	—	D°
,0	,38	1 – –	13.6	62000
,75	1.63	— —	— —	D°
,0	1.38	1 – –	11.5	50000
,77	2.67	— —	— —	D°

~~In the~~ Next the bit of wood was placed between Dd & the projecting brass

,93	1,15	1 — —	14,6	68000
~~,0~~	~~03~~	~~D°~~	~~34.2~~	~~6 of mot [illegible]~~

In all the foregoing 1713 of air consisting of 1 part inf. & 2 of common was used but in the 2 next 2ce that quant was used

,0	,03	1 —	14.6	68000
,0	,13	1 —	13.6	62000
,0	,10	D° with 1713		

Continuat D° Aug. 15. P.136

index bef	after	weights on board	dist	weight	
0	.75	1 —	$11\frac{1}{2}$	50000	1 inf
0	.3	1 —	12.5	55800	3 com
0	.13	1 —	13.5	61400	
0	.23	— —	-	D°	1 inf
0	.70	1 —	12.5	55800	2 com
0	.50	— —	—	D°	inf .1713
0	.29	1	13.5	61400	com. 4176
0	.05	—	—	D°	inf 1 com. 3

Continuat. D° Aug. 16 ~~with [illegible]~~

	.33	1 —	13.5	1 inf. 2 com
	.75	—	12.5	
	1.05	—	D°	1 inf $2\frac{1}{4}$ com.
	.12	—	13.5	
	.03	— —	D°	1 inf 3 com.
	.70	—	12.5	
	.31	— — —	D°	2 inf 3 com
	.10	— —	13.5	
Aug. 17	1.30	— —	11.5	1 inf 3.6 com
	.10	— —	12.5	
	.41	— —	12.5	
	.24	— —	11.5 weak el	1 inf $1\frac{1}{4}$ com.
	.53	—	D° strong	
	.12	—	D° weak	
	.12	—	D° stronger	

Aug. 18 The same repeated only an alterat. made in the manner of firing namely the spark was given to a small tinplate lying at one end on the firing ap:par & resting on glass at the other the ~~four also were charged~~ straw electrom. was also used & the jars were either one of the 4 old jars of my making or the white cyl.

		wei	dist	weight equiv		dist of straw	
0	.70	1	$11\frac{1}{2}$	50000	w cyl	$1\frac{3}{4}$	inf. 1
	143	—	—	D°	D°	$4\frac{1}{2}$	com $1\frac{1}{4}$
	with	air from	zinc				
	.70	—	—	D°	D°	$1\frac{3}{4}$	inf 1
	.60	—	—	—	D°	$4\frac{1}{2}$	
	.45	—		—	D°	D°	com. $1\frac{1}{4}$
	.65	—		—	D	$1\frac{3}{4}$	
	.45	—	—	—	jar	$1\frac{3}{4}$	
	.80	—	—		D°	$4\frac{1}{2}$	
	.48	—	—		D°	D°	
	.86	—	—	—	D°	$1\frac{3}{4}$	

It does not seem therefore as if there was any connection between the strength of the spark & of the explosion

1.40	—	—	D°	w. cyl	2	inf. 1
.74	—	—	—	D°	D°	
.25	1	$12\frac{1}{2}$	55800	D°	D°	com. 3.6
.10	—			—	D°	

P. 137

Continuat		D° ex. same as before		
1 ,18	2+3+4	14.75	45500	1 inf 6 com
1.74	— —	12.75	38800	
,65	— —	13.75	42300	

There seemed to be some minute interval of time between the elect. spark & the explosion which I did not perceive in the former exper.

1.18	—	13.75	42300	equal quant inf & com
,80	— —	14.75	45500	
,70	— —	D°	D°	

There seemed less interv. between spark & explosion

Explosion of deph. & inflam. air

Aug. 25 the test of deph. air burnt by phosph. mention'd P 57 & 100 was 3.820 tried by dist. water some of this was mixed with rather more than $1\frac{1}{2}$ of com. air its test was then 2,152 tried also with dist. water

3954 of this air was mixed with 3424 of inf. air from zinc & fired as usual With weight 57000 on it it sprung up .15 in one trial & ,25 in another

P.138

2 of ~~this deph. air wa~~ the above mentiond deph. air was mixed with 3 of com. air 1500 of this air being mixed with 2100 of inf air tried in usual manner with weight 57000 sprung up ·19 ·10 the burnt air from one of the trials measured 820 its test was ·075

2100 of this air with 1500 of inf sprung up {·15 & burnt air meas 680 / ·16 ——— 680} the test was ·463

2 parts of this with 1 of inf sprung up ·22 ·43 & meas 880 880 its test was 1.044

2 of com. with 1 of inf sprung up {·25 ·53} & meas 1000

some of the unmixed deph air with rather more than $\frac{1}{2}$ of inf sprung up ·25

P. 140

Sept 1 3 measures of com. air were mixed with 3 measures of air phlo:
:gisticated by liver of sulphur whose test was ·064 & 3 parts of this mixed air were mixed with 1 of inf. air from zinc it sprung up {·10
·22
·23
·23

with 3 of com. & 1 inf it sprung up -
- {·28
·23

Sept. 5 48 gra. of steel filings yielded 15900 gra. of inf. air 5 gra. remaining undissolved

Sept. 6 1 measure of air from iron filings ~~bottling~~ mixed with 3 of common air & tried it sprung up {·38
·42
·20
the burnt air of one of the trials measured 1130 its test tried Sept. 8 was ·319

1 measure of air from steel filings with 3 of com. it sprung up {·74
·18
·38

P.141

The air of one of the trials measured 1135 its test was .367

Sept. 9 0

One part air from iron filings & 3 of com.

sprung up	.33	1130 —	.358
	.15 measured	Test	.34
	.33 —	— — — —	

One part of air from steel filings & 3 of com.

sprung up	1.00	1140 — —	.308
	.62 meas	1140 Test	.290
	.63	1145 —	.315

(I have placed after these the previous experiments made in June, as connected with those which follow in September. Ed.)

Dephlogisticated air from solut. ☿ in aq. fort

Some ☿ was dissolved in aq. fort & sufferd to crystallize the remainder was evap. & more ☿ dissolved & proceeded in that manner till almost the whole was turned into crystals The matter procured this way was several times the weight of the aq. fort

500 gra. of this matter was put into small white glass retort in reverb. & the air caught from it in 3 bottles the 1st measured
the 2nd & the 3rd about
The capac. of the retort was about 3400 gra. The test of the diff. parcels was as follow the 1st bott. being called B1 &c

$2 B^3 + 1$ nitr $= 293.5$
1 com. $+ 1 B^3 = 104$
1 com $+ 1 B^2 = 104.5$
1 com $+ 1 B' = 204$
$1 B' + 1 N = 109$

June 16 1781 The matter remaining after former distillat. was mixed with some more of the sol. ♀ the whole together weighing 967 gra. & was put into a barom. tube properly bent the ball being almost filled by it & ~~distil. put into pot~~ heated in pot sand in blast furnace ~~The bulk of air & test in the different bottles~~ was as follows

Bott	bulk air	test
1	4600	2C + 18° = 199
2	4550	===== 180
3	1800	===== 305
		~~70.3~~ / 39.7 / 12 / 110 ‖ 390

There first came over 2 bottles containing together 9150 of N. air 1 meas. of the first added to 2 meas. of common in Fontanas tube occupying 199 & the 2nd tryed in same manner occupying 180 the 1st bott. being not so pure as the 2nd by being mixed with the common air in the appr There then came 6

bottles of dephlogist. air ~~the 1st of them~~ P 56
containing in all ~~about~~ near 40.000
The 1st bott. containd 1800 Its test tried
by adding N. air by 1 measure at at time
to 1 meas of this in Fontana was
70.3|39.7|12|110||390* tried with tub water Though the
bent tube was full of red fumes while it
came over & consequently it must be
mixed with some N. air † next page

June 17 The test of the air of the 4th bott
tried as above was 64.8|31|12|109||391
Its test by new Eud. large bott. was 5.088
both tried with distilled water.

3193 of this air were put into a bott.
& a bit of my fathers phosphorus stuck
on pointed glass kept in it for a
day when it was found that only
25 gra. were absorbed

The same day ~~9486 of~~ a piece of
the same phosph. was put in same
manner into the remainder of the

---

* The last number is the total diminution

P. 57

of this bott measuring 9486 & was fired by making it touch the side

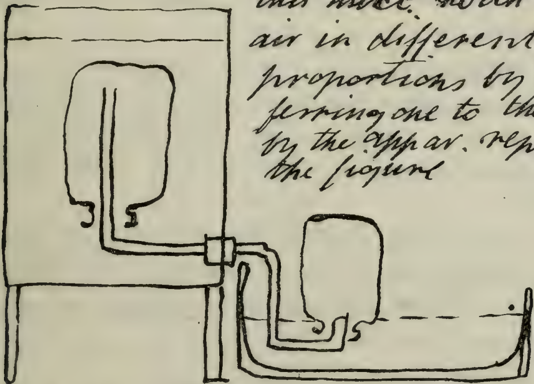
To P. 100

+ Prec. Page) The air of the remaining bottles came over very cloudy except the last which was very little so the last but one also was much less so than the others it good deal of ♀ came over in the process & some red & yellow matter sublimed into the bent tube The ball was found after the oper. was over to be quite shrivelled & compressed by the weight of the sand but very little matter was found remaining in it

& heating it in that situation
it burnt furiously with very
white flame & produced thick
fumes The next day it was found
that 15.25 were absorbed its test
by new Eud tried as before was
4.038 A bit of the new phosph.
was then kept in it for about
12 hours it did not seem dimin
*This air was removed
into bott which when
full weighd 15200 &
with the air 7837
cont[g] air = 7363

*(These five lines are in pencil, Ed.)

~~June~~ June 19 in morn the air in the 2nd. 3 & 5th bottles were put together & some of this mixt. with N. air in different proportions by transferring one to the other by the appar. repr. in the figure



& ~~in~~ the afternoon the test of these mixtures were taken the quantities of the airs were ascertaind by weighing the bottles under water

~~The depth. air transferd~~ into bott containing the N. air

depth	~~airs~~	mixt	contents bott. N air	
899	1162	636	3500	4.485 tub wat 4.46
1062	2556	473	3500	3.98 tub wat ~~[illegible]~~ 3.9

The test by Font. tube in foregoing manner with dist water was 66.6 | 35 | 81 || 3.19

P. 10 2

deph	N. air	mixt	cont. bott	Test			P.103
1298	4794	204	7900	178.7	177.3	122.7	

with dist water

The test of the unmixed deph. air with tub water was 4.982

N.B It was neglected to shake the mixtures ~~after~~ except during mixing so that it is supp. that the mixtures may have diminished after they were made

June 20 some more mixtures of the same deph. air were made by transferring the N. air to the deph. & their test found in the afternoon

deph	N. air	mixt	cont. bott. deph air	Test
1969	4603	144		

1 meas. com. air added to .785 of this in Fontanas tube occupied 1.645 therefore this mixt was rather nitrous

1473	2078	663		4.21 tub 4.296 dist
884	967	515		4.412 dist 4.348 by tub with only 726 of mixt
2058	3,808	466		

Its test with dist. in 2nd bott. was 2.908 therefore remainder was .652

Its test with D° in large bott. ~~with~~ .547 of mixt. was 3.834

N. B These mixtures were shook a good while after they were taken off the pipe but yet by the quantities used in test it should seem as if they had diminished after weighing

June 22 some ~~more~~ mixtures were made & their test taken next morn

deph. air added to N. air

deph	N. air	mixt	cont. bott. in which mixt is made	Test
891	1061	573	1700	4.596
1327	3182	505	4100	3.714
1850	6890	526	7800	

1 meas. com. air added to .542 of this mixt in Font. = 1.17 therefore very nitrous

N. air added to deph.

862	703	585	1700	4.647
1299	1827	537	2100	4.301
1869	3611	362	2550	3.063 in mid. bott.

the test of unmixt air was 4.955 by dist
4.9 by tub

all tried by dist. water except the last

N. B all these mixtures were transferd into bottles whose weight was known in order to find whether the bulk of the mixtures diminished between the weighg & finding the test but by accident some air of all the mixtures except the

1st & 4th was lost the 1st was found
by that means to have diminishd
28 & the 4th 26

The N. air used in these exper.
made very little cloud with lime
water & the deph. air scarce any

June 23 – 2418 of the deph. air
used in the foregoing exper. was
diminishd by liver of sulphur
by shaking it ~~& loosening the~~
~~stopp~~ briskly & loosening the
stopper now & then while inverted
into a glass of liver of sulphur
in order to let in fresh liver of
sulphur as the air diminished
in ~~less than an hour~~ about an hour or 2 it was reduced
to a small quantity & was pourd into
another bott. & was found to measure
72 The next day this air occupied
in Font. tube 42.5 one meas. of N
air being then added occupied 135.2
conseq. the diminut. was $\frac{10}{58}$ of the
air tried

At the same time 1236 of the same
deph air was diminished by liver

sulphur in same manner only in a less degree the reduced air measured 451 its test by new Eudi with dist. water was 4.482

June 26 Trials of the difference in test of deph. air caused by using dist or tub water or using large or mid bott.

some diff. parcels of deph air were ~~taken & its te~~ mixed together & its test taken

dimin	deph	test	
4.701	·993	4.731	dist wat. large bott
4.571	·995	4.594	tub D°
4.563	·987	4.623	D° D°
4.702	·991	4.744	dist D°

2805 of this air was mixed with 2363 of com: its test was

3.091	·986	3.133	dist large bott
2.94	·995	3.09	tub D°
2.952	.989	2.985	dist mid bott
2.862	·979	2.925	tub D°

as the diff: between dist & tub water seemed greater than usual some com air was tried in small bott. with both waters when it came as follows

1.088	dist
1.033	tub

P.106

1 meas. N. air being put into small bott. filled with dist water lost ·119 on shaking for 1' & ·123 ·100 in 2' ·093

Sept. 1781 The remainder of the crystals of ♀ in aq. fort. were put into retort in order to drive off the N. air but the heat being too great a great deal of the white fumes made in procuring deph. air came over with the N. air. The air caught was nitrous but most likely mixed with deph. air as one measure of this added slowly to $2\frac{1}{2}$ of com. made a dimin. of 1.063 whereas one meas of good N. air added in same manner to $2\frac{1}{2}$ of com. made dimin of 1.245 The matter remaining in retort was red & weighed 1490 grs but I believe was a little moist by a little water having been sucked up on the diminution of the heat This was distilled in barom. tube like the former it yielded 4 botts. of deph. air which together measured without any appearance of nitrous air The test of the air

of these botts. was as follows

	com. ad to N	N to com
1	5.053	3.530
2	5.026	3.535 heat tub = 60
3	5.029	3.552
4	5.007	3.548

The ♀ distilled during the distillat was 1316 There were no white fumes produced during the distillat as in former exp.

~~[illegible]~~ 19500 of this air was mixed with 37000 of air from zinc & almost all of it exploded in globe holding 8800 by exhausting the globe by air pump letting in some of the mixt air & exploding it & letting in more air & continuing till all the air was exploded The app. was contrived so that no water could get into the globe along with the air after all the air was exploded ~~most~~ of the air was saved, without letting any water into the globe by exhausting the large globe fitting it on to the small one & opening the cocks so as to let the air out of the small one into the large one the air procured this way measured 2206 & conseq the whole quant. air in the small globe was 2954 to P.146

This air was saved for examination From P. 108. P.146
The quant. water condensed in the small globe was 30 gra. as appeared by weighing the globe before & after the exper. *

The air saved for examination made lime water a little turbid & was diminished 75 or $\frac{1}{29}$ of the whole thereby its test found by adding it to N was 1.969 & by adding N to it 1.407

* This water was sensibly acid to the tast on saturating it with f. alk it yielded near 2 gra. of nitre

The small globe wanted about ,23 of an inc of being perfectly exhausted before the air was let in to be fired & the large globe wanted about as much before the commu:
:nication was made between the 2 globes
The small globe also was found to be not quite air tight

The deph air used in the foregoing exper. had never been passed through water except in receiving it & had been kept stopt in bottles very nearly full As it was therefore suspected that the acid found in the foregoing exper. might be owing to some acid vapour coming over with the air & not absorbed by the water ~~the~~ some of the air in the remaining bottles of deph air was poured out into another bottle so as to let more water into the bottles & the bottles shook several times & on Sept 27 their test was found as follows B5 – 5.000
2 – 4.960 heat tub = 53
3 – 5.010
4 – 5.008

Sept. 28 B4000 of this deph. air was burnt with 66000 of inf. air from zinc in the same manner as before the air procured in the same manner as before measured 2423 conseq. the whole quant was 3070 its test by adding it to N was 1.310
adding N to 1 — 1.068

The water condensed in the small globe P.148
was 50 grs. it was acid but I question
whether so much so as the former

The remainder of this deph. air
measuring 3226 was phlogist
by liver of sulphur it was reduced
thereby to 157 though a small quant
of com. air was let in by accident
its test was ,262

Oct. 1781 The air was procured
from about 13500 of red lead by
oil vitr. ~~the pro~~ at 6 different times
the proportions which seemed to do
best was 2490 of red lead to
1000 of oil vitr. If the oil vitr. was
strong & the red lead dry it made
a great heat on mixing & immed.
discharged air briskly but if the

P. 149

oil vitr. was diluted even in no greater proport. than that of 1 of water to 7 of oil vitr. it would not discharge air without the assistance of heat It yielded 38300 of air which was reduced by lime water to 32400 Its test by

adding it to N was 4.663
N to it 3.347.

Oct. 20 24100 of this air was exploded as before with 44000 of inf. air from zinc the bott. gaind 34 in weight The liquor caught was acid but made no precip. on adding lime water to it though the lime water was more than suffic. to sat. the acid This liquor sat. with lime water was evap. almost to dryness a very small quantity then decanted & the resid. washed it appeard from thence that there could be very little vitriolic acid as almost all the saline part was contained in the decanted part The clear liquors

~~The air caught measured 3200 gra~~
~~conseq. the whole quant. was 4053~~
~~Its test by ~~adding it to~~~~
~~it to N 2nd bott = 1.003~~
~~sm. bott = .960~~
~~N to it ——— .804~~

were precip. by fl. alk. The earth precip
weighed hardly more than $\frac{1}{3}$ gra.
The air caught measured 3200 gra
conseq. the whole quant. was 4053
Its test by adding it to N in 2nd bott = 1.003
sm. bott = .960
N to it ——— .804

Air from plants P.200

Oct. 24 1782 sunshine some pond weed from Shepherd well was put in inverted jar & some more in ~~inverted~~ large bott. some chickweed also with some other water plants were put in a ~~[illegible]~~ ~~some more in a~~ bott. In the Evening the air was separated & the test tried in 2nd meth. was as follows

pond weed in jar —	2.356
D^o in bott — —	2.528
Chickweed — —	~~2.~~ 2.278

The 3 parcels put together meas. 11650 & lost 675 by washing with lime water The next morn. ~~rather cloudy but~~ now & then a little ☉ ~~they~~ the 2 last were exposed again to ☉ & ~~suffered to~~ with fresh water & suffered to remain all night & next day ~~which was clear~~

Oct. 26 sunshine some more pond

weed was exposed in 2 large jars & in the evening the air from them was collected & mixed together & at the same time that from the old plants was collected & mixed together & all the botts & jars filled with fresh water & exposed The test of the air from the old plants was 2.79 / new plants was 2.547 they did not lose much by washing with lime water

Oct. 30 some sunshine & oct. 31 a good deal In the evening the air was collected & the bottles filled with fresh water & exposed the test of the air from the old plants was 2.97 / new plants was 2.79

Nov. 4 the air was taken from the 2 bottles ~~& the botts filled with beccabunga~~ instead of pond weed its test was 2.553 The botts were then filled with beccabunga the jars remaining as before

P. 202

Nov 8 after 2 or 3 Sunshiny cold days the air was taken the test of that from the jars with pond weed was 2.670 that from the beccabunga was 2.690

Nov. 19 The plants had yielded a good deal of air but ~~the great~~ almost all that from the Beccabun. ~~had~~ was absorbed again The test of ~~this~~ from the Becca was ·70
pondweed 2.706

Explosion of above mentiond air with inflam. air

All the abovementiond airs except those got before Oct. 31 were mixed together & washed with milk lime

Its test in 1st method was 3.595 &
2nd method was 2.710

therefore its standard should be { 3.48
3.623

B102
27600 of this air was mixed with

B.17.
41700 of inf. air & all of the mixture except 26700 exploded as in P.

21 gra. of water was condensed which were very slightly acid to the tast but turned paper tinged with blue flowers evidently red The air caught in large globe was 5000 & therefore the whole ~~burnt air~~ quant. of burnt air was 6781 its test in 1st method was .243 & therefore its ~~test~~ standard was .173

Nov. 29. 1782 18400 of the same deph. ~~was exploded in same manner with 28420 of inf. air procured by same oil vitr.~~ was mixed with 28420 of inf. air procured by same acid conseq. the proport. inf. air was greater than in former exp. in proport. 2842 to 2780 all of this mixt except 3900 was exploded as before the matter condensed was 18 gra. it did not tast at all acid nor turned blue paper at all red even when much diminished by evap. The burnt air caught ~~allowing for~~ was 3500 but allowing for some

p. 204

lost is supposed to be about 6000 ~~its test in 1st method was~~ & conseq. the whole quant. burnt air should be about 8000 its test was in 1st method was .327 ~~What is the reason that its test was greater than before though the quant. inf. air was greater I can not tell~~ *

Tu. Dec. 3 14500 of the same dephl. air (B.18) was mixed with 21500 (B.71 + 1500) of inf. air & exploded 3000 of the mixt remaining not used 4000 of burnt air was caught therefore whole quant = 5300 its test in 1st method was, .184 the condensed matter weighd 13 grn. it did not turn blue paper at all red & had no tast

---

* The reason why the test was greater than before though the quant. inflam. air was greater seems to be that more of the inflam. air was left unburnt in this than in former

Dec. 4 16000 (B. 18 + 1500) of ~~depth~~
The same depth air was mixed with 20800 (B. 71) of the same inf air & all except 9300 exploded the condensed matter ~~[illegible]~~ weighd 1¼ grs. & was considerably acid the burnt air caught was 4300 & therefore the whole quant

---

The burnt air however would not take fire by being pourd out from under water under flame of candle this however will not shew a small quant. of inf. air for 2 parts of phlog. air whose test was ,238 with 1 of infl. treated in this manner would not often take fire though it now & then made a small explosion

9 parts of inf air 20 of com. & 29 of ~~[illegible]~~ above phlog. air being tried in appar. for measuring strength of explosions exploded with brisk spark but not with a small one
9 of inf 20 of com & 58 of the phlog would not explode at all though the app. was pretty dry

P. 205

was 5700 its test in 1st method P. 206
was ·972 The condensed matter
being sat. with sol. sal tart. yielded
$\frac{8}{10}$ gra. of salt which ~~being dried~~
appeard by dipping paper in the
solut. & burning it to be nitre
The salt seemed pretty exactly neutra-
lized

This from blue vitr. (To Page 208 Ed.)

4th distillat deph. air from ☿

Dec. 1782 1324 of ☿ were dissolved as before in 600 of aq. fort it yielded 1285 of crystals

2208 more of ☿ were dissolved in 1150 of aq. fort. it yielded 2675 of crystals In all 1750 of aq. fort dissolved 3532 of ☿ & yielded 3960 of crystals N.B part of the liquor used in the 1st making was added to the 2nd The greatest part of the uncrystallized liquor remaining after 2nd oper. being precip. by f. alk yield 163 of precip ~~being distilled~~ This was distilled in glass retort to turn it into red precip. The heat was too strong at first as some deph air came over along with the nitrous air but it was not continued long enough as great part of the inside was still yellow It was reduced hereby to 3559 1462 of the red part was distilled as before in barom. tube the tube broke ~~after it had~~ by water rising up into it

after it had been kept for a great while of a ~~very consid~~ heat supposed nearly red hot without yielding any air to signify & with only very slight reddish fumes in tube By this means it was reduced to.
on Jan. 11
1428 & it was distilled again in new tube it then yielded 78150 of air no red nor white fumes were seen in glass nor did the air come over at all turbid No air I believe came over till the glass was red hot The whole matter was sublimed. The ☿ distilled weighd 1309 which is 119 less than the red precip. used the weight of the air caught was about 97

Jan. 12 ~~some of this air in~~ the Botts 78 & 93 were filled with this air B.93 holding 10400 & B 78 holding 270 more than B 93 the air of B.93 was lost by accident
920 was then pourd out of B.78

P. 210

& the remainder mixed with
B.22 + 4330 of inf. air that is
9750 of the deph. was mixed with
18500 of the inf. & exploded ~~or not~~
in 2nd small globe the globe being
exhausted to within .63   11 gra.
of matter was condensed which
turned paper tinged with blue
flowers red but was not percept.
acid by ~~putting~~ tasting small drop
taken up by finger The burnt
air received in large globe
exh. to within .83 measured
2100 Its test in 1st meth. with mod. was
1.94 bott. therefore stand = 1.86 ~~because the~~ remainder about
1200 being ~~put~~ tried in measure
of explosions would not take
fire on putting a lighted candle
into some of the remainder it
burnt with an enlarged flame
but without the least explosion

the remainder was not dimin
more than $\frac{1}{90}$ by lime water
In afternoon
The same quantities of deph &
inf with the addition of 2500
of air phlog. by iron filings &
sulphur were exploded the
burnt air measured 3200
its test in 1st meth. by mid Bott = 1,003
therefore stand = ,19 sm. B = ·985
The large globe was exhausted to
within .8 the small globe may
be considerd as perfectly exhausted
~~Baro 28,1~~ as by accident
~~gage 28,05~~ it had been filled
with ~~the same air moistened~~ air
of the same kind as that used
The small globe ~~ex~~cluding the cap
held 8900 (In order to find by exper
how much air there should be in small
globe in order to produce this quant. air
in large globe the small globe was
exch. to within 2,87 & commun. to
large globe exch. to within ,87 which

P. 212

was as near as I could at that time exhaust it the air caught in large globe was 1000

The small globe being then exhausted to within 7,1 & large to within ,8 the air caught was 2190 the air furnished by small globe in this last exp. should be about 1590)

The liquor condensed in this exper. was evidently acid to the tast which the other was not On adding some marble powder to each & after suffering it to remain on it a day or 2 & then filtering & precipitating the earth the earth precip. from the 2nd exper. was evidently greater than that from the first the former precip. when dried weighd about 2/10 of gra. the latter was scarce percept.

Jan. 18 8700 of the same deph. air was mixed with 18500 of inf air & exploded as before the condensed liquor did not at all discolour paper tinged by blue flowers either at first pouring out of globe or after it had been kept a night in open glass

The burnt air caught measured 1500 the small globe having been exhausted to within .35 & the large one to within .55 its test in 1st meth. was .458

~~6200 of deph. this of deph. air compos~~ ~~chiefly of different parcels of this distillat but with a little of the 3rd distillat. were mixed with 3 times that quant. of phlogist. air almost perfectly phlogist. the standard of the mixt. was 1.137 8900 more of the phlog air was then added~~

These 2 exper. of Jan. 12 were repeated with some of the same deph. air but with a little variation in the manner

Jan. 26
The small globe was exhausted within .5 & a mixt. of 870 deph. with 1850 of inf. let in & again exhausted to within .5 1500 of deph. air were then let in & then 12200 of this deph. air with 25900 of inf. air were let in at different times & exploded the burnt air caught was 1500 the large globe being exhausted to within .8

P. 213

its test in mid. bott. 1st meth. was P.214
2.041 stand. = 1.958 the condensed
liquor was consid. acid to the tast &
required 37 gra. of a diluted solut.
sol. tart. to sat. it

Jan. 27 The small globe was exhausted
to within .53 & a mixt. of 1500
of deph. air with 2500 of phlog. air
let in after which a mixt of
1~~1750 of those~~ deph. air with
25000 of inf. air were let in
& exploded the burnt air measured
3960 its test in 1st meth. sm. bott.
was .86 stand. = .78 the condensed
liquor was much more acid than
the former & required 119 gra. of
the same alc. sol. to sat. it ~~Both~~
The liquors of both exper. being then
added together & evap. yielded
2.3 of nitre

(Page 215 is blank. Ed.)

Jan. 3. 1783 some clear filtered lime water was put into the new small globe which was exhausted of air by the air pump & then filled with a mixture of 4 parts inf air & 10 of com. which had been previously washed with milk lime This was sufferd to stay about $\frac{1}{2}$ hour to see whether it would make any cloud in the lime water which it did not It was then exploded & sufferd to remain about $\frac{1}{2}$ hour It made not the least cloud in the lime water The diminut was about .444 of whole which is ~~equal to~~ rather more than the bulk of the inf. air + $\frac{1}{5}$ of the com. On opening the globe & breathing a little into the bott. the lime water immed. became turbid

Jan. 4. 1783 20000 of inflam. air were mixed with 75000 of com. & almost all exploded in small globe. the globe being exhausted after each explosion to about 24 inc or about $\frac{2}{11}$ of whole the test of the last parcel of burnt

air in 1st Meth = ,379 therefore stand =
= ,289 7 gra. of liquor was condensed
which did not at all discolour paper
tinged with blue flowers

16200 of deph. air from red. precip.
of diff. parcels were mixed with
3 times that quant. of air imper:
:fectly phlogist. the standard of
the mixture was 1.137 8900 more
of phlog. air was then added which
made its test very little better than
com. air Jan. 24 75000 of this
mixed air was mixed with 20000
of inf. air & exploded as in former
exper. ~~the air after the last explosion being caught as usual &~~ 12 gra. of
liquor was condensed which did
not at all discolour paper tinged
with blue flowers The air after the
P. 217

last explosion being caught as usual
measured 5100 the large globe being
exhausted to within ·8 its test
was .572 & therefore its stand = ·48

Feb. 6 1783 some clean filterd lime
water was put into small globe which
was then exhausted & a mixt. of 3150
of deph. air from red precip. & 6750 of
inf. air ~~let in It~~ which had previously
been washed with milk lime let in
it made no clouds Being then exploded
a very small cloud was immediately formed in the
lime water which did not increase but
seemed rather to diminish on standing
Feb. 7 the exper. was repeated with
a mixt of 2900 of deph. air & 6200
of inf. the globe having been cleand &
exhausted to within ·75 The same
kind of cloud was produced as before
but excessively slight & less than before
the burnt air measured 900 its test
was ,151 the deph. air used in the exp.
though only a remnant appeard by
mixing it with n. air & taking its test to
be very good its standard being
near 4.7 P. 218

Air from Turbith mineral

Some ☿ was dissolved in oil vitr. & ~~evap. almost~~ the greatest part of the acid evap. This was washd with water till it became yellow & till the washings made not much precip. with f. alk. Being then dried it is supposed to have weighd 1920

This was distilled in a glazed earthen retort till it yielded no more air It yeelded 72600 + B164 + B.165 filled to weight of 18350 in all

The test of the 1st bott. holding 6300 in 2nd meth. was 2.61 of the 2nd B 3.585 & of the last 3.402

482 of ☿ distilled over & 620 of matter remaind the retort & neck about 410 of which was running ☿ the rest was a grey powder seeming to contain much running ☿

Some of this air was exploded with inf air in small bott. The bott. was first exhausted to within about $\frac{6}{10}$ 1500 of deph. air of the 2nd bott. was then let in & afterwards a mixt. of 12800 of the deph. air with 25900 of inf were let in at diff. times & exploded The burnt air measured 2200 its standard was 1.54

The exper was repeated in the same manner the burnt air measured 1900 & its standard was 2.378 The condensed liquor was mixed with that procured from former exp. & sat. with vol. sal amm

Some more of this air was exploded The bott was first exh to about $\frac{1}{2}$ inch & 3000 of deph air then let up after which a mixt. of 25600 of deph air & 51800 of inf. was let up & diff. times & exploded The

burnt air measured measured
3500 & its test in 2nd meth was ·91
conseq. its standard was 1,024

Some solut sal. tart was added
to the condensed liquor of the 2
former exper. which as said before
was sat. with v. alk & heated so as
to drive off the v. alk the liquor was
still alcaline The condensed liquor
of the last exper. was added to it
which mad it acid some more
sal tart. was then added till it
was almost but not quite neutral
being evap. it yielded 4 grs. of
salt which was nitrous & which
appeard by the addition of terra
ponderosa salita to contain
scarce any vitriolic acid & I
believe hardly more than the
sal. tart. it was sat. with

REPORTS

ON

THE STATE OF SCIENCE.

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Report on the present state of our knowledge of Refractive Indices for the Standard Rays of the Solar Spectrum in different media. Presented to the British Association for the Advancement of Science. By the Rev. Baden Powell, M.A., F.R.S., F.G.S., F.R. Ast. S., *Savilian Professor of Geometry, Oxford.*

[With two Plates.]

In submitting to the British Association a Report on this subject, on the present occasion, after having already, from time to time, made various statements relative to such results, both to the Association and in published papers,—it may be necessary to explain, that those former statements embraced only detached portions of the subject, and in many instances contained only first, imperfect, and approximate results, which I was nevertheless anxious to bring forward at the time, in order to afford some means of attempting comparisons with theory. These results have been since rendered more accurate by further repetitions, and some points which seemed doubtful, now cleared up: though I have still to regret that it has not been in my power, as yet, materially to extend the range of media examined. To state, however, the entire series of such results as I can now with any confidence offer, and to collect in one view all the other determinations of the kind as yet known to have been obtained, is the object of the present Report.

The historical memoranda of these researches may be briefly stated by way of introduction.

Fraunhofer was beyond question the first who used the dark lines as points of measurement for the deviations of definite primary rays, in seven kinds of glass, and three liquids. His original memoir appears in the Munich Transactions (*Denk-*

schriften der Academie der Wissenschaften zu München für die Jahre 1814, 1815, band v.). This was translated into French in Schumacher's *Astronomischen Abhandlungen*, Zweites Heft, Altona 1823; and from this last an English translation was made in the Edinburgh Philosophical Journal, Nos. 18 and 19, Oct. 1823.

M. Rudberg (in Poggendorff's *Annalen*, band xiv. § 45, and band xvii.) gave similar series of results for the ordinary and extraordinary refractions, and those along the axes of elasticity of several crystals.

The importance of extending such observations was urged by Sir J. F. W. Herschel, in his Treatise on Light (§ 1117. and 1121.), in 1827. The same recommendation was repeated by Sir D. Brewster, in his Report on Physical Optics to the British Association in 1833 (Second Report, p. 319); and with peculiar force, as coming from a philosopher who had done so much for the determination of refractive powers, and almost everything for the dispersive powers, of a vast range of substances.

This led to the formal recommendation of such researches by the Association (Third Report, p. 473); and a grant was placed at my disposal, for the prosecution of this object, at the Dublin Meeting, 1835. I used every endeavour to carry on the observations; and though I found the difficulties in practice greater than I had anticipated, I was able to present at the Bristol Meeting in 1836, and circulated in print, a series of first approximate determinations of indices for various media, which was afterwards embodied in the memoirs of the Oxford Ashmolean Society. Through the same body I published "Additional Observations," &c. in 1838, containing repetitions of some important measurements, with a view, not only to increased accuracy, but to the settlement of some points which seemed doubtful. Some discussion which took place on these points at the Newcastle Meeting, 1838, led to a further examination; and during the present year I repeated the most important determinations, together with some additional investigations, which were printed by the Ashmolean Society under title of "A Second Supplement to Observations," &c., 1839. These are referred to in the following report under the designation of First, Second, and Third Series, respectively.

The object of this Report is to bring together all the data we at present possess of this kind, in a uniform tabular arrangement, which will comprise,—

1st. Fraunhofer's Indices.

2nd. Those of M. Rudberg.

3rd. Those obtained by myself, collected from my several

papers, taking the mean where the different sets were comparable; and in other cases adopting those which appeared to me most to be relied on.

My observations were made with an apparatus, the essential parts of which are a graduated circle, having the prism at its centre, and a small achromatic telescope with cross wires, and a power of 10 nearly, directed to the prism, and moveable on an arm about the centre, along with the index. The diameter is 10 inches; the limb is divided on silver to 10′, and by two opposite verniers with lenses to 10″. It was originally made and divided by Allan, but fitted up for this special purpose, under my directions, by Mr. Simms. The whole will be directly understood by inspection of the annexed plate. (See Plate I.)

The slit, which is the origin of light, is about $\frac{1}{20}$th of an inch broad, formed by the edges of two brass plates made by Mr. Simms, and inserted in a screen, outside of which is the usual apparatus for throwing the sun's rays into any convenient direction; the prism is placed at about 12 feet distance.

The absolute deviation of each ray is thus observed directly from the zero point, or that which is shown on directing the telescope to the slit. The adjustment for parallelism in the edge of the prism with the slit is easily made, and the position of least deviation found accurately by the cross wires and fixed lines.

From the observed deviations, the indices are deduced by the well-known formula (δ being the minimum deviation, ι the prism-angle, and μ the index,)

$$\log. \mu = \log. \sin. \frac{\delta + \iota}{2} - \log. \sin. \frac{\iota}{2}.$$

For liquids, hollow prisms or troughs of truly parallel plate-glass were employed, the angle of each being determined by a method described in my second paper; a thermometer was inserted, and the temperature of the medium always noted.

In the course of my observations some doubt had arisen as to the exact *identification* of certain of the standard rays, according to Fraunhofer's designation of them, owing to the very defective representations given of them in various optical treatises, which fail to convey the peculiar characteristics which mark the different bands.

Among the larger maps of the spectrum, that in the Edinburgh Encyclopædia (art. Optics, plate 433, fig. 16.) is professedly copied from Fraunhofer's, which is given in Schumacher's Journal, before referred to, (tab. ii. fig. 6.); and this, taken from that in the Munich Transactions (tab. ii. fig. 5.). This last

appears to me superior in delicacy of representation, conveying by shading (which is by no means so good in Schumacher's print) an idea of the relative intensity of the different parts of the spectrum. Both preserve admirably the varied characters of the several groups of lines, and present a faithful picture of the actual object. All this, however, is almost entirely lost in the plate in the Edinburgh Encyclopædia, the execution of which is coarse, and the characters of the different bands ill preserved, especially at G; while two small groups of lines between and below the bands at H, are made so conspicuous as to be mistaken for them. In the plate in the Edinburgh Philosophical Journal (No. 18.), H is distinctly marked at the point midway between the two bands, instead of being opposite the lower.

In the plates in the Munich Transactions, and in Schumacher's Journal, the appearance of the numerous lines about G is beautifully given; and I have closely compared these representations with the actual object, both as seen in the small telescope of my apparatus, and also in one with a power of 20. With this power all the smaller lines are seen as in Fraunhofer's plate, but it is insufficient to resolve the two broad bands. In that plate, however, they are represented as formed of masses of very fine lines close together; and in the less refrangible group, as nearly as possible at its centre, there is one line a little stronger than the rest, opposite to which G is marked.

Thus, the middle of the lower band, in my observations, appears correctly taken for the exact position of G. As some guide to the appearance and position of the lines, I have annexed a map laid down from my own observations, and giving their general character as exhibited in the small telescope of my apparatus (see Plate II.).

With regard to the accuracy of the observations, and the degree of accordance between one set and another, it should be borne in mind, that the liquid prism is necessarily exposed to the heating power of the sun's rays during the whole time of observation. Hence, the refractive power will be liable to continual small changes; for which evil no remedy seems applicable, but that which may be supplied in multiplying observations, from which, it may be presumed, the resulting mean values will furnish determinations on which increasing reliance may be placed. In general, the observations at higher temperatures are less to be depended on.

No comparison can be made between the refractive powers of different media, until some means can be found of reducing them to a common temperature. I find a *proportional* diminution

of the indices, for an increase of temperature will not hold good except within very confined limits.

In many media the violet and blue rays are absorbed; and in others the lines are very faint, or invisible: in some such cases, however, their position might be estimated nearly by means of coloured glasses. Some results of this kind I have stated, distinguishing them as only rough approximations, in cases where, from the nature of the substance, we can hardly hope to obtain more accurate results. Among those media of this class which I have recently examined, is the liquid ammonia; the volatile nature of the substance being such as to occasion so great a want of homogeneity that no lines are visible, and the measures are only rough estimations.

In some cases, and those among the most interesting, not even such approximate measures appear attainable. This was especially the case in that highly important instance, the chromate of lead. A good specimen of this crystal was kindly presented to me by H. J. Brooke, Esq., F.R.S., which was (not without considerable difficulty) cut into a prism of small angle by Mr. Dollond. But the appearance of the spectrum was altogether confused, and the blue end wholly absorbed, so that no measures could be obtained.

In some of my first series of observations, no distinct measures were taken for the two bands at H, but the middle point between them was taken. Such results are rejected in the present report. But in the low-dispersive cases, the difference is so minute, that I have thought it quite sufficient to introduce a small *proportional* correction, to reduce the index to the exact position of H.

Some observations given from the first series, for certain chemical solutions of very low dispersive power, are of little value, since the differences of temperature render comparison impracticable, though the whole subject of the indices of chemical compounds, compared with those of their elements, is one well deserving more full investigation.

Results for a few other media are added from observations *now first published*; among which will be found rock-salt, so interesting from its relations to heat.

I had entertained hopes of being able before this time to obtain results for a much more extended range of substances, especially those of the more highly dispersive class. But there are many difficulties in procuring specimens in a state susceptible of this mode of prismatic examination; there are also many substances, and among them the most important, to which it is to be feared such examination cannot be applied.

My present Report will therefore be found to embrace only a small number of media of the more important class, but in which, after repeated examination, I think the results may be relied on.

Table I.

Indices determined by Fraunhofer.

B.	C.	D.	E.	F.	G.	H.
I.—Flint-glass, No. 13. Specific gravity, 3·723. Temp. 15° Reaum. = 18°·75 Centig.						
1·627749	1·629681	1·635036	1·642024	1·648260	1·660285	1·671062
II.—Ditto, No. 23. Sp. gr. 3·724. Mean of observations with two different refringent angles.						
1·626580	1·628460	1·6336665	1·640519	1·646768	1·6588485	1·669683
III.—Ditto, No. 30. Sp. gr. 3·695.						
1·623570	1·625477	1·630585	1·637356	1·643466	1·655406	1·666072
IV.—Ditto, No. 3. Sp. gr. 3·512.						
1·602042	1·603800	1·608494	1·614532	1·620042	1·630772	1·640373
V.—Crown-glass. M. Sp. gr. 2·756.						
1·554774	1·555933	1·559075	1·563150	1·566741	1·573535	1·579470
VI.—Ditto, No. 9. Sp. gr. 2·535. Temp. 14° R. = 17°·5 C.						
1·525832	1·526849	1·529587	1·533005	1·536052	1·541657	1·546566
VII.—Ditto, No. 30. Sp. gr. 2·535.						
1·524312	1·525299	1·527982	1·531372	1·534337	1·539908	1·544684
VIII.—Oil of Turpentine. Sp. gr. 0·855. Temp. 8°·5 R. = 10°·6 C.						
1·470496	1·471530	1·474434	1·478353	1·481736	1·488198	1·493874

TABLE I. (*continued*).

IX.—Solution of Potass. Sp. gr. 1·416. Temp. 9° R. = 11°·25 C.						
B.	C.	D.	E.	F.	G.	H.
1·399629	1·400515	1·402805	1·405632	1·408082	1·412579	1·416368
X.—Water. Sp. gr. 1·000. Mean of two experiments. Temp. 15° R. = 18°·75 C.						
1·330956	1·331711	1·333577	1·335850	1·337803	1·341277	1·344169

TABLE II.

Indices determined by M. Rudberg.

I.—Calcareous Spar. [Prism-edge parallel to axis of rhombohedron.] Ordinary ray.						
B.	C.	D.	E.	F.	G.	H.
1·65308	1·65452	1·65850	1·66360	1·66802	1·67617	1·68330
II.—Ditto. Extraordinary ray.						
1·48391	1·48455	1·48635	1·48868	1·49075	1·49453	1·49780
III.—Quartz. [Prism-edge parallel to axis of rhombohedron.] Ordinary ray.						
1·54090	1·54181	1·54418	1·54711	1·54965	1·55425	1·55817
IV.—Ditto. Extraordinary ray.						
1·54990	1·55085	1·55328	1·55631	1·55894	1·56365	1·56772
V.—Topaz. 1st axis of elasticity. [Ray in direction of axis.]						
1·61791	1·61880	1·62109	1·62408	1·62652	1·63123	1·63506

Table II. (*continued*).

VI.—Topaz. 2nd axis. [Ray in direction of axis.]						
B.	C.	D.	E.	F.	G.	H.
1·60840	1·60935	1·61161	1·61452	1·61701	1·62154	1·62539
VII.—Ditto. 3rd axis. [Ditto.]						
1·61049	1·61144	1·61375	1·61668	1·61914	1·62365	1·62745
VIII.—Arragonite. 1st axis. [Ditto.]						
1·52749	1·52820	1·53013	1·53264	1·53479	1·53882	1·54226
IX.—Ditto. 2nd axis. [Ditto.]						
1·68061	1·68203	1·68589	1·69084	1·69515	1·70318	1·71011
X.—Ditto. 3rd axis. [Ditto.]						
1·67631	1·67779	1·68157	1·68634	1·69053	1·69836	1·70509

Table III.

Indices determined by the author of this Report.

I.—Double-distilled Oil of Cassia at 10° Centig. [3rd series.]						
B.	C.	D.	E.	F.	G.	H.
1·5963	1·6007	1·6104	1·6249	1·6389	1·6698	1·7039
II.—Ditto at 14°. [3rd series.]						
1·5945	1·5979	1·6073	1·6207	1·6358	1·6671	1·7025
III.—Ditto at 22°·5. [2nd series.]						
1·5895	1·5930	1·6026	1·6174	1·6314	1·6625	1·6985

TABLE III. (*continued*).

IV.—Sulphuret of Carbon at 15°·65. [Mean of 2nd and 3rd series and No. ii. of 1st.]						
B.	C.	D.	E.	F.	G.	H.
1·61823	1·62190	1·63083	1·64386	1·65550	1·67993	1·70196
V.—Oil of Anise at 15°·1. [Mean of 2nd series, No. iv., and 1st series, Nos. iii. and iv.]						
1·54865	1·55080	1·55725	1·56590	1·57435	1·59120	1·60842
VI.—Ditto (probably altered) at 13°·25. [3rd series.]						
1·5482	1·5504	1·5565	1·5650	1·5733	1·5901	1·6066
VII.—Ditto at 20°·9. [Mean of 1st series, Nos. i. and ii., and 2nd series, No. iii.]						
1·54507	1·54730	1·55345	1·56235	1·57077	1·58815	1·60527
VIII.—Kreosote at 18°·2. [Mean of 3rd series and 2nd and 1st, No. i.]						
1·53196	1·53353	1·53833	1·54523	1·55153	1·56390	1·57486
IX.—Oil of Sassafras at 17°·2 [Mean of 1st series, No. ii., and 3rd series.]						
1·52575	1·52750	1·53215	1·53870	1·54485	1·55750	1·56935
X.—Sulphuric Acid. Sp. gr. 1·835. Temp. 18°·6. [From 1st series.]						
1·4321	1·4329	1·4351	1·4380	1·4400	1·4440	1·4463
XI.—Muriatic Acid. Sp. gr. 1·162. Temp. 18°·6. [Ditto.]						
1·4050	1·4065	1·4095	1·4130	1·4160	1·4217	1·4261

TABLE III. (*continued*).

B.	C.	D.	E.	F.	G.	H.
XII.—Nitric Acid. Sp. gr. 1·467. Temp. 18°·6. [From 1st series.]						
1·3988	1·3998	1·4026	1·4062	1·4092	1·4855	1·4206
XIII.—Alcohol. Sp. gr. ·815. Temp. 17°·6. [Ditto.]						
1·3628	1·3633	1·3654	1·3675	1·3696	1·3733	1·3761
XIV.—Pyrolignous Acid. Temp. 16°·2 Sp. gr. 1·060.						
1·3729	1·3745	1·3760	1·3785	1·3807	1·3848	1·3884
XV.—Concentrated solution of Pure Soda. Temp. 16°. Sp. gr. 1·34.						
1·4036	1·4039	1·4075	1·4109	1·4134	1·4181	1·4221
XVI.—Solution of Caustic Potassa. Temp. 16°. Sp. gr. 1·42.						
1·4024	1·4036	1·4061	1·4091	1·4117	1·4162	1·4199
XVII.—Rock-salt.						
1·5403	1·5415	1·5448	1·5498	1·5541	1·5622	1·5691
XVIII.—Solution of Muriate of Lime. Temp. 22°·2. [1st series.]						
1·4006	1·4016	1·4040	1·4070	1·4099	1·4150	1·4190
XIX.—Solution of Muriate of Ammonia. Temp. 22°·2 [Ditto.]						
1·3499	1·3508	1·3529	1·3552	1·3575	1·3617	1·3650
XX.—Solution of Nitrate of Potassa. Temp. 22°·2. [Ditto.]						
1·3457	1·3468	1·3487	1·3510	1·3533	1·3586	1·3608

TABLE III. (*continued*).

XXI.—Solution of Sulphate of Magnesia. Temp. 22°·2. [1st series.]						
B.	C.	D.	E.	F.	G.	H.
1·3434	1·3442	1·3462	1·3486	1·3504	1·3540	1·3570
XXII.—Solution of Nitrate of Mercury. Temp. 21°·6. [Ditto.]						
1·3408	1·3419	1·3439	1·3462	1·3487	1·3528	1·3560
XXIII.—Solution of Muriate of Barytes. Temp. 21°·8. [Ditto.]						
1·3398	1·3406	1·3421	1·3438	1·3466	1·3504	1·3531
XXIV.—Solution of Sulphate of Soda. Temp. 22°. [Ditto.]						
1·3392	1·3398	1·3419	1·3442	1·3462	1·3499	1·3528
XXV.—Solution of Muriate of Zinc. Temp. 22°. [Ditto.]						
1·3351	1·3402	1·3421	1·3444	1·3466	1·3501	1·3534
XXVI.—Solution of Nitrate of Bismuth. Temp. 22°. [Ditto.]						
1·3306	1·3315	1·3332	1·3355	1·3374	1·3410	1·3437
XXVII.—Solution of Nitrate of Lead. Temp. 17°·8. [Ditto.]						
1·3455	1·3461	1·3482	1·3506	1·3528	1·3568	1·3600
XXVIII.—Solution of Superacetate of Lead. Temp. 19°. [Ditto.]						
1·3429	1·3437	1·3455	1·3480	1·3498	1·3538	1·3571
XXIX.—Solution of Subacetate of Lead. Temp. 15°. [Ditto.]						
1·3350	1·3357	1·3373	1·3398	1·3417	1·3453	1·3481

Table III. (*continued*).

XXX.—Distilled Water (the same in which the above solutions were made). Temp. 15°·8. [1st series.]						
B.	C.	D.	E.	F.	G.	H.
1·3317	1·3326	1·3343	1·3364	1·3386	1·3429	1·3448
The same with a *proportional* reduction for temperature, to compare with Fraunhofer's at 18°·75 Centig.						
1·3310	1·3320	1·3336	1·3357	1·3380	1·3412	1·3441
Approximate Indices, in cases where, from the nature of the substance, it does not seem likely that more accurate measures can be obtained. XXXI.—Balsam of Peru at 19°·2. [Ditto.]						
1·585	1·587	1·593	1·603	1·613	1·634	1·653
XXXII.—Oil of Pimento at 19°·8. [Ditto.]						
1·528	1·532	1·535	1·542	1·548	1·559	1·571
XXXIII.—Oil of Cummin at 22°·1. [Ditto.]						
1·502	1·504	1·507	1·513	1·520	1·532	1·543
XXXIV.—Oil of Angelica at 21°. [Ditto.]						
1·484	1·486	1·489	1·493	1·496	1·505	1·509
XXXV.—Solution of Chromate of Lead in Nitric Acid. Temp. 18°·6. [Ditto.]						
1·369	1·372	1·374	1·376	1·379	1·384	1·389
XXXVI.—Solution of Chromate of Potassa. Temp. 20°·2. [Ditto.]						
1·351	1·352	1·353	1·357	1·360	1·364	
XXXVII.—Liquid Ammonia. Sp. gr. 898. Temp. 15°.						
1·345	1·346	1·348	1·350	1·353	1·355	1·360

Oxford, August 22, 1839.

Report on the application of the sum assigned for Tide Calculations to Mr. WHEWELL, *in a Letter from* T. G. BUNT, Esq., *Bristol.*

[With two Plates.]

Bristol, August, 1839.

DEAR SIR,

I SEND you eleven new tide-sheets, Nos. 33 to 43, containing discussions of my new tide-gauge observations made between September 1837 and June 1839, together with two of the former sheets, Nos. 25 and 29, showing the correction-curves of lunar parallax and declination from the dock observations of 1836 and 1837. I have also added several new correction-curves to those of *lunar* parallax and declination merely, on sheet No. 29, arising out of my investigations of the solar effects on the times of H.W.; and my numerical calculations, undertaken for the purpose of reconciling the discrepancy between the declination-curves, numerically and graphically obtained. My suspicions fell on the latter, as I have already informed you; but after a long and laborious scrutiny I found that the others were most in fault. All the lunar correction-curves hitherto obtained were, however, defective, for want of applying the solar correction, the introduction of which has considerably altered and, I hope, improved them.

From the *unexplained residues* of time and of height contained in the sheets for 1834, 5, 6, and 7, (44^h anterior epoch,) I obtained the *corrections for solar effect*, first in a series of 24 curves for half calendar months, and afterwards for $\left\{\begin{matrix}10^\circ\\22^\circ\end{matrix}\right\}$ declination and $\left\{\begin{matrix}8''\cdot45\\8''\cdot70\end{matrix}\right\}$ parallax. The solar correction was also found by means of the numerical calculations of the times of H.W. for 1834, 5, 6, 7, and 1838, which was calculated twice. The results may be seen in sheet No. 29. Another arrangement was made from the residues of time on the sheets for 1834, 5, and 6, for the declination corrections of 5°, 13°, 20°, and 25°. The results, which are laid down on sheet No. 29, strongly favour the supposition that the effects of the declinations are as their squares. My latest declination-curves on sheet No. 37, for 8°, 19°, and 26°, are still more decisive; the sum of the twelve ordinates between the lines of 8° and 19° being to those between 19° and

26° as 57 : 58 by measure on the sheet, and as 57 : 60 by the law of the squares; differing only $\frac{1}{30}$th from the law.

In hope of throwing some light on the question of the best anterior epoch of *declination*, considered separately from parallax, I arranged the numerical errors, or residue of calculated times for 1837 and 1838, keeping the increasing and decreasing declinations separate, but not distinguishing north declinations from south. I was not, however, able in this way to perceive how any change in the declination epoch would be attended with advantage.

An arrangement of the numerical errors in the calculated times for 1837 into two parcels for each month, those following a north transit in one parcel, and those following a south transit in the other, gave the following results:—

1837. Month.	Mean Error of calculated time of H. W. following a *North* Transit. Dock Observations.		Mean Error of calculated time of H. W. following a *South* Transit. Dock Observations.	
	min.		min.	
January	3·7	too early.	3·7	too late.
February	2·4	ditto.	2·4	ditto.
March	1·6	ditto.	1·6	ditto.
April	2·3	ditto.	2·3	ditto.
May	2·0	ditto.	2·0	ditto.
June	1·9	ditto.	1·9	ditto.
July	1·4	ditto.	1·4	ditto.
August	3·0	ditto.	3·0	ditto.
September	2·3	ditto.	2·3	ditto.
October	2·5	ditto.	2·5	ditto.
November	1·6	ditto.	1·6	ditto.
December	2·3	ditto.	2·3	ditto.

The average of the year gives the observed time of the H.W. following a *north* transit, 2·2 minutes *later* than the mean or calculated time; and of the H. W. following a *south* transit, 2·2 min. *earlier* than the mean.

It is quite certain that the diurnal inequality in the intervals at Bristol is such as to make the time of a H.W. next after a north transit, in the main, *later* than that of a H. W. next after a south transit. This is evident upon the slightest inspection of sheets 37 to 43 tide-gauge observations.

The effect of this *constant* of diurnal inequality is positive when the moon's declination (four days anterior) is north; and negative when it is south; or, in other words, the diurnal inequality in the interval is greatest when the moon has [had]

north declination, and least when south. Is it possible to refer this to anything but the moon's difference of distance from the two opposite surfaces of the ocean?

I next proceeded to discuss the observations of the times of H.W. from my tide-gauge register, using the mean between two equal altitudes, taken at $\frac{1}{4}$th of the distance from H.W. to mean water, instead of the actual or observed time of H.W. In the first discussion of about twelve months' observations in this way, made between Sept. 1837 and Jan. 1839, and laid down on sheets 33 to 37, the anterior epoch of 44 hours was employed as formerly. A comparison of the lunar correction-curves from the different epochs of 32^h, 44^h, and 56^h, seemed to indicate that the epoch of 38^h, intermediate between those of 32^h and 44^h, would probably afford correction-curves of parallax and declination approximating more closely to each other, both in form and in magnitude. At your request, I made the trial first with about six months' observations, which, being treated in the usual manner, yielded, after several approximations and a new solar correction, curves of lunar declination and parallax of the shape that had been anticipated,—the second loop in the declination-curves (at 8^h transit) diminishing, while that in the parallax-curves (at the same hour of transit) was increasing. An improvement was also seen about the hours 1 and 2 of transit; the mean error was at the same time lessened. I next tried (by your directions) the effect of this change of epoch on the whole of the tide-gauge observations I possessed, equal to about those of one year in all, and laid them down in a second series of curves on the same sheets, Nos. 33—36. The solar and lunar corrections were approximated several times, and those finally obtained are given at the bottom of sheet No. 33. On trying the mean error taken at every hour of transit throughout the whole series of observations, it was found almost identical with that before obtained from the same observations, with the old epoch of 44 hours, viz. $2\frac{1}{2}$ min. very nearly. You then requested me to make a further trial, in order to determine, if possible, whether the new epoch was better than the old one. To do this properly, I found that it would be necessary to draw the curves of observation afresh, and to interpolate the times of transit for every 6 and 18 hours between those given in the Nautical Almanac with greater nicety, namely, with second differences and decimals of minutes. I therefore made the necessary corrections in the intervals, and laid them down anew. You know we had proposed to use a larger number of observations in our second trial of the comparative merits of the two epochs; but as it appeared, upon further consideration, that this would not

give a fair comparison, unless the same extension of the observations were also tried with the old epoch, which would have greatly added to the labour, I determined to confine myself to the same observations while using the epoch of 38 hours as had been employed with the epoch of 44 hours; which was, in fact, to go over the ground a second time with the shorter epoch, endeavouring to avoid any small errors I might have incurred before, such as that I have just explained. The corrections I eventually obtained, and laid down on sheet No. 37, are, I believe, for the moon's effects a fourth approximation, and for the sun's a third. On carefully measuring the mean residual error after the two processes, I find a *small* improvement in that of the 38^h epoch, which is now 2·394 min., while that of the 44^h epoch is 2·510 min.

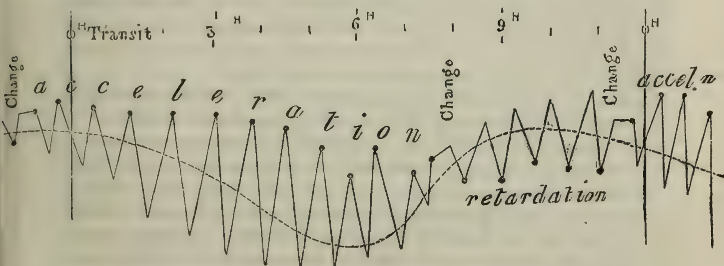
Whatever may be thought of this minute difference, which accidental circumstances may have in some degree modified, I think no one can hesitate to give to our new curves of lunar parallax and declination a preference before those numbered 1 and 1 on sheet 29, which were considered so good only twelve months ago. A discussion of heights would probably afford additional evidence in favour of our new epoch; as there is little doubt that the maximum height would fall much nearer than formerly to the hour of 0^h transit; though I believe it will now be a little to the *right hand* of that hour line, as though the epoch were rather too much shortened. If similarity of form between the parallax and declination correction-curves be admitted as the proper test of the anterior epoch, it is pretty evident that 38^h is better than any other that we have yet tried.

The observations of displacement of summit of the tide-curves, or differences of equal altitude-times, and actual times of H. W., are laid down on sheets Nos. 33—36, and the resulting corrections on No. 33. The observations are continued from January to June, on sheets Nos. 42 and 43. I had transferred all these observations to a separate sheet, and begun to discuss them afresh: but seeing no reason for thinking I could throw any further light on the subject just at present, I laid it aside until more observations had been made, or until you had favoured me with your opinion as to the best mode of proceeding.

You will be pleased at hearing that Dr. Carpenter's exertions, so ably seconded by your letter, have succeeded in procuring a grant of 50*l.* from the Corporation, in addition to a previous one of 20*l.* from the Society of Merchants, since increased to 25*l.*, making together 75*l.*, which has been paid me in consideration of my time and labour bestowed on attending to the tide-gauge, and calculating a tide-table annually for the port.

These grants are donations only: but it has been intimated to me that they may be repeated.

A few days ago I laid down the Bassadore intervals, which you have copied into your paper, for the sake of looking at the enormous diurnal inequality which appears in them. I have not the diagram at hand, but I remember being struck with the great disparity between the *acceleration* of the time of H. W. after a given transit—say the superior,—when the moon was on *one* side of the equator, and the *retardation* when she was on the *other*; thus:—



These observations having been made in the month of November, a part of this disparity may perhaps be due to the sun, which has then considerable south declination; but if the same thing be observed in the summer months, shall we not have here another case similar to that of Bristol, where the H. W. next after a *south* transit is, about three times out of four all through the year, *earlier* than that after a *north* transit, as I have already remarked?

I am, Dear Sir, &c.
T. G. BUNT.

In the Philosophical Transactions for 1839, Part I. page 151, will be found a further discussion of observations of high and low water at Plymouth, showing that the height of *mean water* at that place is nearly permanent; also a comparison of the high and low water at Leith, showing that at that place the height of mean water is still more nearly permanent, the variation being only a very few inches.

The accompanying curves (Plates III. and IV.) exhibit the results of the *equal altitude* observations referred to in Mr. Bunt's letter. The *times* of H. W. here employed are not the moments when the water is observed to be highest, but the mo-

ments obtained by bisecting the times at which the water is at three-fourths the height of H. W. above the level of mean water, during the rise or the fall of each tide. The corrections are arranged according to the hour of the moon's transit, and represented on a scale of forty minutes to an inch. The transit of the moon here employed is not the one immediately preceding the H. W., but, in the first figures, it is the one a day and a half previous to that which precedes the H. W.; that is, the transit B of Mr. Lubbock. In the latter figures, constructed from *thirty-eight hours' anterior epoch*, an epoch or transit is employed, interpolated midway between B and C, which appears in some respects to give better results than either of those two transits.

The curves entitled *Displacement of Summits* express the difference of time of H.W. as actually observed, and as inferred by bisection of the interval of equal altitudes just described. This difference shows itself in the displacement of the summits of the curve, which exhibits the rise and fall of the water on Mr. Bunt's machine. It is a remarkable and not very easily explicable circumstance, that this displacement appears to be more affected by solar parallax than by any other element. When the displacement is such as to *accelerate* the time of H. W., the fall of the tide is less rapid, and when such as to retard the time of H.W., the fall is more rapid than the rise.

W. Whewell.

Notice of Determination of the Arc of Longitude between the Observatories of Armagh and Dublin. By the Rev. T. R. Robinson, D.D., *&c.*

At the Edinburgh Meeting of the Association, a Committee was appointed to determine, by chronometers or signals or both, the longitudes of the principal observatories of the British Isles, and its members were authorized to apply to the Government for any aid that might be necessary. Cambridge and Oxford present no peculiar difficulty, but the observatories of Ireland and Scotland, both from distance and local circumstances, are less easily managed. The chronometric part of the process has, however, been most effectually performed by one of our members, Mr. Dent, who, in the first instance, sent twelve of his chronometers from Greenwich to Edinburgh and Makerstown, (the observatory of Sir Thomas Makdougall Brisbane): they were returned again to Greenwich, and Professor Henderson has deduced from the results,

	m s
Longitude of Edinburgh	+ 12 42·99
Makerstown	+ 10 3·66

These results were reported at the Newcastle Meeting by Sir Thomas Brisbane, and they inspired Sir W. Hamilton and myself with the desire of obtaining a similar determination of our longitudes. Mr. Dent not merely placed these chronometers at our disposal, with three additional, but bestowed what was even more precious, his personal attendance, and assisted us in the comparisons; an advantage which could not have been purchased, but which I notice as an instance of the aids which these meetings afford to Science.

Twelve of the chronometers were rated as before at Greenwich Observatory, the three others at the Marine School; and Mr. Dent, setting out on the evening of September 20, was enabled by the rapidity of railroad travelling to compare them at Dublin on the morning of the 22nd, and on that of the following day at Armagh, having travelled about 500 miles. His return was effected with equal rapidity, and I have deduced from the comparisons,

	m s
Longitude of Dublin	+ 25 21·08
Armagh	+ 26 35·44

which, however, cannot be regarded as definitive until the personal equations of the Irish observers shall have been compared with those of Greenwich.

The extreme consistency of the individual results, the greatest difference being $1^{s}\cdot65$, is well calculated to inspire confidence, and there is every reason to rest satisfied with these numbers, as the chronometric longitudes.

Yet, however accurate they be, it is impossible to consider the means by which they are obtained as superseding the method by signals. The first, transports the time from one station to another by machines, which, though their performance is wonderful, yet must be disturbed by that very process; in the second, the chronometer is light. Its application is far more costly as well as difficult, but its certainty is greater, and the whole of the disturbing causes are in view. The general character of it is this,—The flame of powder at an intermediate station is observed from the observatories, and the difference of the times is that of longitudes. If, however, the interval is too great for one signal, two with an intermediate observer are employed: the eastern signal is observed by him and the eastern observatory; a short time after he observes the western signal in conjunction with the west observatory, and the longitude is the difference of observatory times, lessened by that which has elapsed between the two signals. Thus any number of intermediate stations may be employed. The powder is generally fired on mountains, and it is found that the flash of small quantities is visible at very great distances. Four ounces have been seen at 140 miles. When mountains are not to be found, the requisite height must be gained by rockets; and an elegant application of this is seen in Sir J. Herschel's operations for determining the relative longitude of Greenwich and Paris, detailed in the Philosophical Transactions for 1826.

This kind of signals is essential in Ireland, and even with them the local circumstances of Armagh are such as to present great difficulties. A range of hills rises to the south, from 600 to 1000 feet above it, at about four miles distance, and these are shut out from Dublin by high ground to the north of it. I was deprived of the aid of Colonel Colby, by his absence in Scotland, where he was engaged in making the necessary arrangements for the completion of its survey, but my friend Lieut. Larcum supplied all necessary information, guided by which the mountain Slieve Gullion was selected. Its summit is visible from Dublin at fifty-two miles, but is 800 feet below the land which bounds my view, and this decided the size of the rockets necessary, as, besides certainly clearing that ridge, they were to carry four ounces

of powder. The two-pounder is necessary for this, and, on stating my objects, a liberal supply of them was ordered by the Board of Ordnance, together with tents for the firing parties; and, indeed, whatever I required was freely afforded. Without dwelling on details, it may be sufficient to mention that on four nights in May last, notwithstanding most unfavourable weather, we obtained from seventy-four signals, forty-two good results: from which we deduce our difference of meridians to be

$$1^{m}\ 14^{s}{\cdot}425;$$

only three-hundredths of a second greater than Mr. Dent's chronometers had given.

In taking this mean, it is necessary to attend to the different value of the work of each night, which varies according to the number of signals observed, and that of the stars observed for time. This has been done according to the theory of probabilities, in applying which it was found that the probable error of time-determination by a star is about 0·065, and by a rocket 0·16. It also appears, that when several intermediate stations are used, the value of the result is rapidly diminished; so that, for example, if as in Sir J. Herschel's operations between Greenwich and Paris, we suppose three stations—ten signals at each, and seven stars to determine time at the extremities, the worth of such a result is but 0·38 of what it would be if the work could be done by one signal station. If to this we add the great uncertainty of perfect transmission along the line, it becomes an object to increase the extent of distance commanded by each signal as much as possible.

To complete the result, it is necessary to know the "personal equation" of the observers, or, in language fit for the uninitiated, the difference of the times at which two observers estimate the passage of a star over a transit-wire,—such a difference as astronomers well know almost always exists, and sometimes to a startling extent. By a journey to Dublin, my assistant determined that he observed 0·167 earlier than the other observer, and therefore the true difference of longitude is

$$1^{m}\ 14^{s}{\cdot}258$$

It is our intention next, to determine the longitude of the third great Irish observatory, that which Mr. Cooper is furnishing with instruments of unexampled magnitude and power, which can be connected by one station with this and Dublin simultaneously. That I hope to follow up by a similar operation between Armagh and Edinburgh, if, as I expect, the Board of Ordnance prove as propitious to my second application. Rockets of sufficient power fired on Goatfell in Arran, can be

seen by these observatories, distant 105 and 85 miles. They must, however, rise 1100 yards, while those used by me ascended from 600 to 800 yards only; but this is a range quite within reach of Woolwich pyrotechny. Several which I lately made, not exceeding two pounds, rose with the same heading, from 1200 to 1500 yards; and, judging from the range of English war-rockets, their ascent would be even greater. If they be supplied, it would be an object of no common interest to see the instruments of carnage and terror dovoted to the ministry of science. The flash of powder can be seen at even greater distances than those named, but its flame is far less brilliant than many other pyrotechnic compositions, some of which I find are thrice as luminous. If this attempt succeeds, the junction of Dublin with Oxford, by signals on the Welsh mountains, is not more difficult; and perhaps even the connection of Greenwich and Paris, by a single station, is not impossible.

T. R. Robinson.

Report of some Galvanic Experiments to determine the existence or non-existence of Electrical Currents among stratified Rocks, particularly those of the Mountain Limestone formation, constituting the Lead Measures of Alston Moor. By H. L. Pattinson, *Esq.*

At the meeting of the British Association held last year in Newcastle-upon-Tyne, I had the honour of being intrusted with a grant of money for the purpose of making some experiments to determine if any appreciable galvanic action existed among stratified rocks, but I had more immediately in view to experiment upon the alternations of strata found in the Lead Districts of Northumberland, Cumberland, and Durham. There was associated with me in this inquiry my friend, Mr. Thomas Richardson of Newcastle, but he was prevented giving me his assistance at the time required. Mr. John Leathart of Alston then offered to assist me, and I gladly availed myself of his cooperation, the more valuable because his residence on the spot enabled him conveniently to make some necessary arrangements which would otherwise have occasioned me much trouble.

In the Lead District to which I have alluded, the mountain limestone alternates with indurated clay and sandstone (called technically Plate and Hazle,) with remarkable frequency and regularity. A section of the strata in this part of the country was published in 1800 at Carlisle, by W. Miller, miner, but a far more complete and accurate section was published by Mr. Westgarth Forster in 1809, which is to be found also in the forty-fifth volume of the Philosophical Magazine. The following statement of the 'lead measures' of Alston Moor is copied from Mr. Forster. Its general correctness I have had frequent opportunities of verifying.

	yds.	ft.	in.		yds.	ft.	in.
Fell top limestone	1	1	6	Brought forward	29	0	2
Coal	0	0	8	Upper slate sill	8	0	0
Hazle, or upper coal sill	4	0	0	Plate	2	1	6
Plate	10	0	0	Lower slate sill	7	0	0
Hazle, or whetstone sill	3	0	0	Plate	10	0	0
Plate	4	0	0	Hazle	3	0	0
Hazle	4	0	0	Slate	7	0	0
Plate	2	1	0	Iron stone and coal	1	1	6
Carried forward	29	0	2	Carried over	68	0	2

	yds.	ft.	in.
Brought forward	68	0	2
Fire stone	11	0	0
Plate	8	0	0
Hazle	3	2	0
Plate	4	1	0
Hazle	2	0	0
Plate	4	1	0
Hazle, called Pattinson's sill	4	0	0
Plate	6	0	0
Little limestone	3	0	0
Plate	6	0	0
Sulphureous coal	0	1	6
Hazle, called High Coal Sill	4	0	0
Plate	2	1	6
Sulphureous coal	0	1	0
Hazle, called Low Coal Sill	3	1	0
Plate	6	0	0
Great limestone	21	0	0
Carried forward	158	0	2

	yds.	ft.	in.
Brought forward	158	0	2
Hazle, called the Tuft	3	0	0
Plate	7	0	0
Limestone	0	1	6
Quarry hazle	10	0	0
Plate	11	0	0
Harder Plate, called Till Bed	2	1	6
Four fathom limestone	8	0	0
Nattrap Gill hazle	6	0	0
Plate	11	0	0
Three yards limestone	3	0	0
Six fathoms hazle	12	0	0
Plate	3	1	6
Five yards limestone	2	1	6
Slaty hazle	4	0	0
Plate	6	0	0
Scar limestone	10	0	0
Plate	1	0	0
	259	0	2

In considering these numerous alternations it is easy to imagine that they in some measure resemble a voltaic pile, and the conjecture not unnaturally arises, that possibly some galvanic action may be exerted among them. It is easy to speculate further, that the veins traversing these strata perpendicularly, may possibly act as electrodes, and that their metallic and other contents differing so much from the rocks in which they occur, may be drawn together and collected by galvanic currents excited by the strata and circulating in the veins. These views and suppositions are not at all new; they have been put forth on different occasions in various publications of the day, but as far as I know, up to the present time, have never been submitted to the test of experiment. I mean the galvanic action of stratified rocks has not been before tested by experiment, for Mr. Fox has undoubtedly shown that galvanic currents do exist in some veins.

I have briefly to lay before the Association an account of the experiments performed by my colleague and myself on this subject, which, if they have not cleared up the matter, have at least drawn the uncertainty within smaller bounds.

Our first experiment was to determine if any difference existed in the electrical condition of the limestone stratum, called the *Great Limestone* in Forster's section, and a soft sandstone, called the *Tuft*, lying immediately under it, the under surface of the limestone and the upper surface of the sandstone being

in contact. The experiment was performed in an adit or level, driven partly in each of these strata, at a mine called White Well, near Alston, and the spot selected was at a distance from any vein. We proceeded by fixing, about twelve inches above the under surface of the limestone, in holes bored for the purpose, twelve hollow copper cylinders, each five inches long by one inch diameter, which were firmly secured in the holes and forced strongly into contact with the rock by means of wooden plugs driven within the cylinders. With these cylinders a copper wire one twentieth of an inch in diameter was connected, and one end of this wire was attached to one end of the wire of a delicate galvanometer. In the same way twelve copper cylinders were inserted in holes made in the sandstone, about a foot from its upper surface, and to these were attached a similar wire, which was brought in contact with the other end of the galvanometer wire, so that any current between the two strata would be immediately perceived by its action upon the needle. On making the contacts, no motion whatever was produced on the needle, and every expedient for increasing the effect of a feeble action was resorted to, such as making contact at short and equal intervals, the interval being the time required for one vibration of the needle, which was previously determined. At the same time the sensibility of the instrument was such, that a plate of zinc and a plate of copper, each one eighth of an inch square, in pure water produced a most distinct action. The galvanic action, if any existed between this limestone and sandstone stratum when the experiment was performed, was, as collected by the metallic surfaces in contact with each, decidedly less than that of a pair of zinc and copper plates, each one eighth of an inch square, in pure water.

The next experiment was upon a stratum of limestone and a stratum of hazle, having a plate bed between them, and the situation selected was a shaft near Alston, called Water Greens shaft, sunk by the Commissioners of Greenwich Hospital some years ago into Nent-force level. The strata sunk through in this shaft, as shown in the Hospital books, are as below; it is in the lower part of the series just given from Forster's section, the beds differing a little from the thicknesses stated by him, as they almost always do when measured at different places.

	yds.	ft.	in.		yds.	ft.	in.
Clay	10	1	0	Brought forward	31	0	0
Part of six fathoms hazle	7	2	0	Plate	3	0	0
Hard plate	1	0	0	Scar limestone	18	0	6
Five yards limestone	3	1	6	Hazle	0	2	0
Hard plate	0	1	6	Plate	6	1	6
Hazle	8	0	0	Hazle	2	0	0
Carried forward	31	0	0		61	1	0

The limestone pitched upon for this experiment was that called the *Five Yards Limestone*, and the sandstone the *Six Fathoms Hazle*, the plate bed between them being three feet in thickness and a little more strongly indurated than the plate beds in the district usually are. There was a flooring or *bunyan* across the shaft at this place, and a level or drift driven out of the shaft to a small distance, in which the galvanometer was placed. The situation was at a distance from any vein, and all the circumstances were of the most favourable nature for performing the experiment with the utmost completeness and precision.

Six holes, each two feet deep, were bored in the sandstone a foot above its under surface, and six similar holes were bored in the limestone a foot below its upper surface; into these holes were introduced twelve slips of sheet copper, each two feet long by two inches wide, bent so as to be semi-cylindrical, and they were fixed and *stemmed* very tightly into the holes so as to secure the most perfect contact. The six copper slips in the hazle were connected with a copper wire one twentieth of an inch in diameter, the end of which, as before, was attached to one pole of the galvanometer, and the six coppers in the limestone were similarly connected together and brought in contact with the other pole of the galvanometer. Every possible care was taken in attaching the wires and in making the contacts, but the needle of the galvanometer did not show the slightest current, although at the same time it was fully sensible to the action of the plates of zinc and copper, one eighth of an inch square, in pure water. When the contacts were made at intervals so as to be isochronous with the times of vibration of the needle, and continued for several minutes, there was still no appreciable effect whatever.

It was thus fully established that under the circumstances detailed, there was no current given off equal in amount to that excited by a pair of plates of zinc and copper, one eighth of an inch square, in pure water; indeed, when we reflect upon the thickness and imperfectly conducting nature of the strata, no very obvious current could be expected. It is, however, still possible that, by using a more delicate galvanometer, bringing into contact with each stratum a larger surface of metal, and including a greater number of alternations of strata within the circuit, a current may yet be detected, and the matter is certainly open for further investigation.

Bensham Grove, Gateshead. August 24th, 1839.

Report respecting the two series of Hourly Meteorological Observations kept in Scotland, at the expense of the British Association. By Sir David Brewster, K.H. LL.D. F.R.S. L. & E.

Having fixed upon Inverness and Kingussie as two suitable stations for carrying on the two series of hourly observations, which I undertook to establish and superintend for the British Association, I was fortunate in being able to prevail upon the Rev. Mr. Rutherford of Kingussie, and Mr. Thomas Mackenzie, teacher of Raining's school, Inverness, to undertake these observations. The instruments which were necessary for this purpose were made by Mr. Adie of Edinburgh, under the superintendence of Prof. Forbes, and the observations commenced on the 1st of November, 1838, the beginning of the Meteorological year, or the first of the group of winter months. I directed the two observers to pay particular attention to the *auroræ boreales*, and to record every phænomenon of this nature; and I have no doubt, from the lists already sent me, that this class of observations will be the most complete and valuable that has ever been made in Scotland.

I annex a specimen of the observations made at Kingussie at the time of the great depression of the barometer on the 29th November, 1838, which will exhibit the nature and value of the register.

As it is of the greatest importance to obtain the true curve of the daily variation of temperature and pressure at the two stations of Inverness and Kingussie, the last of which places is between 700 and 800 feet above the level of the sea, I earnestly hope that the Association will permit these observations to be carried on for at least another year.

An Extract

An extract from the Register of the Thermometer and Barometer kept at Kingussie by the Rev. A. Rutherford.

Attached Therm.	Hour.	Therm.	Barom.	Weather.
	27th November, 1838.			
37°	1 A.M.	27°·75	28°·880	Clear, calm.
36	2	27	28·846	ditto
36	3	28	28·834	Clear, windy, W.
36	4	27·75	28·810	ditto
35	5	27·75	28·776	ditto
36	6	29·50	28·734	ditto
35	7	30·50	28·680	ditto
35	8	31·50	28·676	ditto, wind S.
36	9	32·75	28·636	ditto
36	10	34	28·620	ditto
36	11	34·50	28·608	ditto
36	12	35·25	28·580	ditto
36	1 P.M.	36	28·560	ditto
37	2	36·50	28·538	ditto
37	3	35·75	28·478	ditto
36	4	35	28·464	ditto
38	5	35	28·474	ditto
38	6	36	28·480	ditto
37	7	36	28·464	ditto
38	8	35·50	28·524	ditto
38	9	36	28·432	ditto
39	10	36·25	28·454	Partly clear, calm.
39	11	36	28·428	ditto, gusts of wind
39	12	35·75	28·426	ditto
	28th November, 1838.			
39	1 A.M.	36	28·368	Cloudy, windy, S.E.
39	2	37	28·364	ditto
38	3	37·50	28·340	ditto
38	4	38	28·316	Cloudy, calm.
38	5	38·50	28·286	ditto
41	6	37	28·268	ditto
40	7	38·75	28·220	ditto
40	8	38·50	28·230	ditto
40	9	38·50	28·196	ditto
40	10	4.050	28·142	Cloudy, S. E.
41	11	41·50	28·108	ditto
42	12	41·50	28·054	ditto
42	1 P.M.	40	28·084	Rain, windy, S. E.
41	2	40·25	27·938	ditto
42	3	41·25	27·882	Fair, ditto
42	4	41·50	27·826	ditto
42	5	41·75	27·812	ditto
42	6	40·25	27·672	Rain, windy, S. E.
42	7	39·75	27·624	ditto
42°	8 P.M.	39°·05	27°·582	Rain, windy, S.E.
44	9	41	27·488	ditto
44	10	41·75	27·460	ditto
43	11	42	27·424	ditto
43	12	43	27·426	ditto
	29th November, 1838.			
43	1 A.M.	43·50	27·438	Partly clear, wind S.E.
44	2	44	27·412	ditto
45	3	43·75	27·396	Cloudy, squalls.
44	4	43·50	27·390	ditto
44	5	43·50	27·386	ditto
44	6	43	27·336	ditto
45	7	42·50	27·364	ditto
44	8	42·50	27·334	ditto
44	9	41·50	27·334	ditto
45	10	42.75	27·334	Rain, wind S. E.
45	11	42·75	27·292	Cloudy, ditto
45	12	42·25	27·264	ditto
44	1 P.M.	42	27·220	ditto
44	2	44	27·208	ditto
45	3	44	27·200	ditto
45	4	44	27·216	ditto
45	5	43·75	27·220	ditto
45	6	43·75	27·220	ditto
45	7	43	27·232	ditto
45	8	43·75	27·244	Cloudy, wind S. E.
45	9	43·50	27·280	ditto
46	10	42·75	27·270	ditto
45	11	43	27·270	Light clouds, S. E.
45	12	43·50	27·264	ditto
	30th November, 1838.			
45	1 A.M.	43·50	27·236	Cloudy, calm.
45	2	43·50	27·264	ditto
44	3	42	27·256	Raining, ditto
44	4	41	27·268	ditto
44	5	41·50	27·312	Fair, ditto
46	6	43	27·326	ditto
46	7	43	27·346	ditto
46	8	43·50	27·492	Drizzling, wind W.
46	9	After this the bar. rose rapidly to its usual height.		ditto
46	10			ditto
46	11			ditto

Note at the end of November.—From the 27th till the 30th only ½ inch of rain fell, notwithstanding the low state of the barometer. The depression seems to have been more the result of high gusty winds, and probably falls of rain, or other commotions in the neighbourhood, than any immediate fall here.

Notes of the Appearance of the Aurora Borealis.

13th November, 2 o'clock, A.M., aurora till 6 o'clock, A.M. The following are the more minute details.

Having been awakened by the assistant at a quarter to 2 A.M., I found a rather bright aurora, consisting of an arched bend from N.E. to N.W., with a few streams of light darting up to the zenith. There were also a few light clouds floating along the northern horizon, with a haze of light on the back ground. At 3 A.M., streaming up very bright, bend as formerly, light clouds over the aurora. At $\frac{1}{4}$ past 3 A.M., very bright. Besides the arched bend formerly mentioned there was another dimmer arch higher up, extending from a little to the North of East, quite across the sky to a little North of West, its upper border reaching the seven stars or pleiades. Between these two arches there were a great many spires of light darting up and recoiling, with frequently a sort of waving flash running over the slightly illumined parts, and even where no spires of light were visible, except where the flash gave them a momentary existence; clouds on the horizon, but not many. 25 min. to 4 A.M., a few clouds on the horizon, and a bright arch above them, but few spires of light. $\frac{1}{4}$ to 4 A.M., as last noted, but streaming more up to the zenith. 4 o'clock, A.M., a few clouds in the N.W. horizon; the light dim, few spires, and these extending only a short way up the sky. $\frac{1}{4}$ past 4 A.M., as last noted, with an arch of light a little above the horizon, right over North, and spires of light darting up. $\frac{1}{2}$ past 4 A.M., ditto, ditto. $\frac{1}{4}$ to 5, A.M. few clouds, but a dark colour below the arch near the horizon. 5 A.M., clear coruscations flashing up to the zenith, the upper edge or arch passing through it, or nearly so, from N.E. to about due W., touching Orion's belt with its upper border. Flashes of light rolling from W. to E., illuminating lines of the aurora perpendicular to the lower arch, and reaching the higher one, but only rendered visible by the coruscations passing over them. The aurora brightest where there are clouds on the horizon. No wind nor frost, although clear, except the few clouds mentioned. $\frac{1}{2}$ past 5 A.M., coruscations from the horizon up to the zenith in a hobbling manner. 6 o'clock, only a dim light.

17th November, 1838, 10 o'clock, P.M., slight aurora, but not visible at 11 P.M.

14th December, ditto. 9 o'clock, P.M., slight ditto, tops of the spires only visible above the horizon in the north. 11 P.M. only a dim light.

St. Leonard's, St. Andrew's. August 22nd, 1839.

Report on the subject of a series of Resolutions adopted by the British Association at their Meeting in August, 1838, *at Newcastle, to the following effect*:—

"Resolved. 1. That the British Association views with high interest the system of simultaneous magnetic observations which have been for some time carrying on in Germany and various parts of Europe, and the important results to which they have already led, and that they consider it highly desirable that similar series of observations, regularly continued in correspondence with, and in extension of, these should be instituted in various parts of the British dominions.

"2. That this Association considers the following localities as particularly important:—Canada, Ceylon, St. Helena, Van Diemen's Land, and Mauritius or the Cape of Good Hope; and that they are willing to supply instruments for their use.

"3. That in these series of observations the three elements of horizontal direction, dip, and intensity, or their theoretical equivalents be insisted on, as also their hourly changes, and on appointed days their momentary fluctuations.

"4. That the Association considers it highly important that the deficiency yet existing in our knowledge of terrestrial magnetism in the southern hemisphere should be supplied by observations of the magnetic direction and intensity, especially in the high southern latitudes between the meridians of New Holland and Cape Horn, and they desire strongly to recommend to Her Majesty's Government the appointment of a naval expedition expressly directed to that object.

"5. That in the event of such expedition being undertaken it would be desirable that the officers charged with its conduct should prosecute both branches of the observation alluded to in Resolution 3, so far as circumstances will permit.

"6. That it would be most desirable that the observations so performed, both at the fixed stations and in the course of the expedition, should be communicated to Professor Lloyd.

"7. That Sir J. Herschel, Mr. Whewell, Dr. Peacock, and Professor Lloyd be appointed a Committee to represent to Government these recommendations.

"8. That the same gentlemen be empowered to act as a Committee, with power to add to their number, for the purpose

of drawing up plans of scientific co-operation, &c. relating to the subject, and reporting to the Association.

"9. That the sum of 400*l.* be placed at the disposal of the above-named Committee for the above-mentioned purposes."

The Committee named in the above-mentioned Resolutions report,—

That in pursuance of the instructions conveyed in Resol. 7, they immediately proceeded to address a copy of the Resolutions to His Grace the Duke of Northumberland, President of the Association, for signature, with a request that he would forward it so authenticated to Lord Melbourne, accompanied by a letter from Sir J. Herschel, soliciting an interview on the part of the Committee, for the purpose of entering into the necessary explanations. His Grace having most readily complied with this request, a reply was after some time received from Lord Melbourne to the following effect:—

"SIR, "Windsor Castle, September 3, 1838.

"I BEG leave to acknowledge your letter of the 27th ult., and to acquaint you that I shall be most happy to receive the Committee of the British Association for the Advancement of Science upon my return to London; and remain, Sir,

"With great respect,

"Your faithful and obedient Servant,

"MELBOURNE."

"*Sir J. Herschel, Bart.,*
10, *Hanover Terrace, Regent's Park.*"

By a subsequent communication from his lordship the interview in question was deferred until the Chancellor of the Exchequer could appoint a day for attending. At length Saturday the 10th November was appointed; but the last mentioned minister not being then able to attend, his lordship declined considering the interview as official, allowing the Committee however to place in his hands for consideration a memorial, of which the following is a copy.

Memorial of the Committee appointed by the British Association for the Advancement of Science, to submit to Her Majesty's Government the Resolutions of that Association on the subject of Terrestrial Magnetism.

The instruments and the methods of observation hitherto employed in determining the dip and variation of the magnetic needle, whether at fixed stations, or in surveys and voyages of

discovery, have been such as to afford results incapable of being depended on to any minute degree of precision, calculated only to satisfy the immediate practical wants of the ordinary navigator; and, so far as theory is concerned, to give a general view of the course of the magnetic lines in those parts of the globe which have chiefly been the scenes of inquiry, and to establish the important facts of changes more or less rapid taking place in the intensity and direction of the magnetic forces at every point of its surface.

Of late, however, methods infinitely more perfect have been devised and practised in Germany (whence their use has extended to other nations in Europe), which have given to magnetic determinations a precision previously supposed to be unattainable—a precision not inferior to that of astronomical observation. The time is therefore now arrived when all that is rude and inexact in the subject of terrestrial magnetism must give place to rigorous numerical statement and refined discussion; and, when a theory can exist, which, like those of the planetary movements, basing itself on a few perfectly ascertained elements, shall embrace in general formulæ all the intricacies of the subject—define *à priori* the course of the magnetic lines over the whole surface of the globe, and retrace or predict their variations for centuries past or to come.

To ascertain these elements with all the strictness which human skill and industry can command; to investigate, by systematic, and, when necessary, by co-operative observation, those inductive laws which must serve as the stepping stones of such a theory, and to amass a series of well concerted and effective observations, of sufficient accuracy and extent to serve as tests of its truth—have appeared to the British Association for the Advancement of Science objects of such eminent importance as to justify it in recommending them to Her Majesty's Government, as worthy of some considerable national exertion and expenditure. Private research has done much, and is doing more, and the practical bearings of the subject may possibly stimulate commercial bodies to afford some slight and incidental aid in the inquiry; but there are features in the case which lead us to conclude that without the co-operation of Government on an extensive scale, the resources of science must be employed in vain, or at a disadvantage which must retard indefinitely the desired consummation.

For, in the first place, the observations necessary for the purpose in view require to be made at many very distant points of the globe, for a considerable length of time, and (what is particularly to be noticed) at moments strictly simultaneous.

This precludes all hope that individuals going about the world from station to station may, by slow degrees, at length amass the information required. It has been ascertained that the magnetism of the globe is liable to sudden and transient disturbances, which, so far as appears, are strictly contemporaneous over great districts, and not improbably over the whole surface of the earth. Hence arises the necessity of employing no observations but such as are strictly simultaneous in combinations destined to elicit the values of the magnetic constants, since otherwise the resultant of forces in action at a given point of the globe would be brought into comparison with that of forces which have ceased to act in another point, and thus the influence of local situation would become confounded with that of temporary change.

Observations prosecuted on such an extensive scale, and so strictly in concert, are altogether beyond the reach of individual zeal and enterprise. They demand a systematic organization and official responsibility quite as much as an outlay of funds, and it is with a view to this that the Association has agreed to the 1st, 2nd, and 3rd Resolutions, which recommend the establishment of what may be called *Primary Magnetic Stations*, at the places therein named, at points most convenient and appropriate for combination with the European stations already in activity, including those of Greenwich and Dublin.

In concert with such primary stations, it would be both natural and highly desirable that travellers provided with the requisite instruments, or officers in other stations who may be willing to devote a portion of their time to this service, and who may for that purpose be temporarily provided with the instrumental means, should act. Every such primary station then, supposing such to be established, would henceforth become a point of reference and comparison, by which short and desultory series of observations in other localities might be rendered available; including under this head such as might be made in the course of nautical surveys and voyages of discovery, or where from other causes it might be impracticable to remain for any considerable time. And it must not be forgotten that from the very peculiar nature of the case, the observations at any one such primary station would be a check upon the fidelity of those made at every other By the help also of such corresponding observations, continued with regularity and steadiness during some considerable length of time, we should be able to ascertain whether the means they suggest of determining the differences of longitude of different stations be really practicable and capable of extension over the whole surface of the globe.

Should this turn out to be really the case, it would be a collateral result of no small practical value, being in effect a new and general means of ascertaining the longitude, at least of places on land.

But although the ancient methods of observation are superseded for the nicer purposes of theory, by the more refined methods above alluded to, it by no means follows either that the results to which they have led are to be thrown aside as useless, or the methods themselves rejected, when the object is not to attain the last degree of numerical accuracy, still less when these more refined processes are impracticable, as they are at sea, where, nevertheless, it has been shown by Erman and others that the dip and intensity, as well as the variation, can be observed with a considerable approximation.

It has been, and must ever continue to be, the peculiar province of these ruder ordinary processes, to trace out by actual visitation of every accessible spot on the globe, those important curves which, when laid down on charts, express to the eye general relations, such as theory must exercise itself in rendering account of; and, following them through all their intricacies, must find its first application in showing how they originate in the relations of the forces in action. These curves are in fact no other than approximative expressions of those inductive laws above alluded to, and will play the same part in a strict general theory of terrestrial magnetism which Kepler's laws do in that of gravitation, or the polarized figures do in that of light.

A glance at the best and most approved charts, of variation and intensity, will show how much is wanting to render this precursory knowledge complete. However, in all easily accessible regions of the globe the investigation is proceeding with rapidity, nor is there any desirable information of this nature in such regions, which the diligence of navigators, surveyors and travellers, in the pursuit of their ordinary objects, will not adequately furnish. The case is otherwise with those difficultly accessible regions, in which, unfortunately, lie the most characteristic, critical, and important points and inflexions of the curves in question, viz. the points of greatest intensity, and those in which the needle points vertically downwards, and which are usually known by the name of the magnetic poles. Two such points of greatest intensity are known to exist in the northern hemisphere, one in Siberia, and the other in the territories of the Hudson's Bay Company; the existence of the first has been proved by the voyages and travels of Hansteen, Due, and Erman; and of the other, by the observations made

in the voyages of arctic discovery. In these voyages also, as is well known, one locality, where the dip of the needle is 90°, has been visited and determined by Captain James Clark Ross: and it is the opinion of some philosophers that this may not be the only point in the northern hemisphere where the direction of the dipping needle is vertical, and the intensity of the horizontal force consequently evanescent.

But, when we turn to the antarctic regions, we find nothing decisive or determinate. There are no continents admitting of overland exploration, and the seas, which must be the scene of inquiry, are visited by no commercial enterprise, and traversed by no casual vessels belonging to the British or any other navy. Nevertheless, it is vain to hope for a complete magnetic theory till this desideratum be supplied. The unsymmetrical form of the magnetic curves will baffle every attempt to reduce them under general laws, and will remain, as at present, an object of idle wonder, until these points, which may be looked upon as the keys to the enigma they offer, shall have been ascertained. It is on this ground that the Association have agreed, in their 4th Resolution herewith submitted, to recommend to Her Majesty's Government the appointment of an expedition expressly destined to the investigation of the magnetic phænomena of the antarctic regions.

The magnetic pole (or poles) of the southern hemisphere are, in all probability, inaccessible, but this does not prevent their situations being ascertained with tolerable precision by the convergence of the magnetic meridians in their neighbourhood; taken in conjunction with observations of the dip; by occasionally landing and observing on the ice out of the reach of the ship's attraction; and by exploring, as far as possible, all those localities in which the needle may be observed to change its direction with rapidity.

On the other hand there is reason to believe, from Major Sabine's report to the British Association in 1837, that the points of greatest intensity *are* accessible, the one lying in somewhere about 50° S., to the south of Van Diemen's Land; the other in about 60° S., about midway between the meridians of Van Diemen's Land and Cape Horn: positions named, however, rather with a view to fix our ideas as to the means of research, than from supposing it possible to speak with precision at present.

These points, it would, of course, be desirable that the vessels of the expedition should endeavour to attain, or at least to approach sufficiently to settle their position and the amount of the maxima of intensity.

In order, however, not to lose the precious opportunity offered by the proposed expedition for placing on record the most exact determinations in localities so remote from ordinary intercourse by simultaneous observations, in correspondence with those at fixed stations, the expedition ought to be supplied with all the apparatus and requisites for such observations.

The laws of magnetism, under any circumstances important to a great maritime power, are every day acquiring additional interest by reason of the introduction of iron vessels into navigation. The practicability of employing these for long voyages will of course entirely depend on that of obtaining a satisfactory correction of the disturbing influence of the magnetism of the vessel. This influence, as appears from the recent experiments of the Astronomer Royal, Mr. Airy, on the Rainbow steamer, of 200 horse power, is enormous, and such as to render the loss of the ship, if hazarded at sea in a long voyage and in bad weather, hardly so much a matter of risk as of certainty. To get rid of this influence is a problem of no ordinary difficulty—a difficulty enhanced by the fact ascertained in the experiments alluded to, of its originating in two distinct modifications of the magnetic power. The difficulty has been overcome, however, by the eminent mathematician just mentioned, and the Rainbow, which before was brought round with much difficulty from Liverpool to London, is now running with the same confidence in her compasses on the part of her commander as would be the case in an ordinary ship, the cause of error being, as would appear, completely counteracted. It is understood that the success of the Rainbow has not only caused several other iron vessels to be built for the navigation of the British seas and the Baltic, but that projects on a far more extended scale are entertained for communication by iron steamers with the most distant regions.

This striking instance may serve, in reply to any objection which may possibly arise to the proposed expedition, as having for its chief and ostensible object the promotion of merely theoretical research. That object, it is true, *is* to perfect a theory, but it is a theory, pregnant, as we see, with practical applications of the utmost importance. The correction operated by Mr. Airy's process is complete (as he has himself shown,) only for that particular magnetic latitude for which the adjustments are made, and those nearly adjacent; and, although he has pointed out the course of proceeding by which it may be hereafter extended, yet the subject is one but just broached,

and awaits its development from every new light which can be thrown upon it, either by voyages abroad or by experiments at home.

It would be taking a very limited view to confine the objects of the proposed expedition to the investigation of one class of phænomena, however intrinsically important. In naming magnetic research as its paramount object, the British Association wish to be understood not as excluding or underrating others, but rather as marking by this emphatic selection, their sense that its adoption would stand justified in the eyes of the scientific world by this object alone. They are, however, quite aware how many others might, and ought to be embraced in the project of such a voyage, which, taken collectively, would in all probability render it as memorable and as glorious as any of those great enterprises in the Northern Seas which figure so conspicuously in the maritime history of this country. In a geographical view, indeed, the discovery of land which may exist yet unsuspected in the Southern Ocean, and the tracing out more perfectly such as has been shown to exist without defining its extent has been considered by the geographical members of the Association so desirable, as would have induced them on that ground alone to have made it a subject of recommendation to Government. And if we do not now dilate on other objects, it is because we know that, supposing the expedition resolved on, upon what we conceive to be its chief and prominent ground, the other great scientific bodies of the country, and especially the Council of the Royal Society, when called upon, can be at no loss to indicate numerous and most important lines of inquiry, and to furnish every necessary instruction for their effectual prosecution.

In urging this subject on the attention of Her Majesty's Government we wish to be understood as fully recognizing the principle of not resorting to national assistance except where the object aimed at is of national importance; where private zeal and private means are already in full activity, and exerted to the utmost; or where other nations have set an example which may justly arouse our emulation. In this case too, we may add, where no private enterprise can accomplish the end proposed. As regards the importance of the subject, our opinion is already stated of all that body of knowledge which, taken collectively, constitutes what the French call *Physique du Globe.* Terrestrial magnetism is the branch which, at the present moment, stands nearest to the verge of exact theory. It is an outpost ready to surrender, if attacked in form, and

that on which our forces ought therefore to be concentrated with a determination to subdue it. As regards the amount of private research already expended on the subject, and still in activity, we may refer generally to the masses of valuable observation collected in Major Sabine's Report. For many of these we are indebted wholly to private zeal and exertion. In cases where the observers have been officers or persons in public employ their services in this respect have been extra-official and voluntary, and in some cases extremely laborious. We may also refer to the discovery of the north-western magnetic pole itself by Captain Ross, in the prosecution of an enterprise which, however peculiar its circumstances, and however unlikely to be followed as an example by others, was yet strictly a private undertaking. Further, we may refer to the elaborate digests of existing knowledge contained in the variation charts of Barlow, and in Major Sabine's recent report, drawn up at the instance of the British Association in 1837; and lastly to the researches now carrying on (also in pursuance of a recommendation of the British Association) by that distinguished officer, in conjunction with Professor Lloyd and Captain Ross, on the magnetism of the British Isles.

As regards the example of other nations much might indeed be said, but we shall only cite one instance, which can call up no ideas but the pleasing ones of admiration and gratitude, viz. that of Norway, which, though a small and poor state, imposed on itself additional burthens (at a time when economy was especially felt to be necessary), by an unanimous vote of its Storthing, for the express purpose of defraying the expenses of that journey of Hansteen, to which we owe his most valuable and important magnetical determinations between the meridians of Greenwich and Okotsk, in the north of Europe and Asia, and from the 40th to the 75th degree of north latitude.

We therefore feel ourselves on strong ground when we call upon Government to aid us in a task where we are actually doing so much, and to lend a crowning hand to so much exertion. Great physical theories, with their trains of practical consequences, are preeminently national objects, whether for glory or for utility. The peace which now happily subsists may not continue many years longer, and in the turmoil of war such objects are little likely to engage attention. The opportunity therefore which now exists for such an expedition may, in the concurrence of events, be snatched from us altogether, whereas at present everything is favourable.

(*Signed on the part of the Committee*,)

J. F. W. Herschel.

On the 29th of November the Committee again waited, by appointment, on Lord Melbourne, and the Chancellor of the Exchequer being now present, the nature and extent of the equipments of the fixed observatories, and of the scientific department of the proposed naval expedition, were more particularly entered into, reference being made for the general views and principles of the project to the memorial above copied, and a further memorandum was handed in, stating more expressly the instrumental and other requisites for the undertaking, and an estimate, founded on the best judgment the Committee were at that time enabled to form, of the probable expense of the fixed observatories, which they were led to rate at about 2000*l*. for each observatory, exclusive of chronometers and transit instruments.

To the representations of your Committee on this occasion every attention was paid; nor did they experience any difficulty in the way of stating their views in the fullest and amplest manner, in a less formal and official mode, either to these or other members of Her Majesty's Government, and in particular to Earl Minto, to whom it was especially requisite that the views of the Association relative to the naval expedition should be clearly developed. To these various representations, for a considerable time, no definite reply was received. Meanwhile the President and Council of the Royal Society, no less impressed than this Association with the great importance of the subject, had deputed a Committee of their own body for the purpose of making a similar application. And your Committee have great satisfaction in being enabled to state, that, aided by the warm and zealous exertions of the Marquis of Northampton, who, whether in the chair of the last mentioned illustrious body, or of this Association, has on all occasions shown himself active in promoting every great scientific object,—these concurrent representations have been attended with their full effect; that every point suggested in the resolutions on which this report is founded has been ordered to be carried out into full execution, and every observation recommended provided for by Her Majesty's Government in the most ample and liberal manner. And that, probably at the very time when this report will be read to the Association, two ships, the Erebus and Terror, will be already on their voyage to the Antarctic Seas, under the command of Captain James Clark Ross, carrying with them every instrument requisite for the complete and effectual prosecution of these important researches, and having on board the complete establishments, both personal and instrumental, of the fixed magnetic observa-

tories at St. Helena, the Cape of Good Hope, and Van Diemen's Land; the two former being under the respective direction of Lieutenants Riddell, Lefroy and Wilmot, of the Royal Artillery; the latter it is contemplated to place under the direction of one of the naval officers attached to the expedition, who will be left in charge of it.

The observatory at Montreal, in Canada, will be placed under the direction of Lieutenant Riddell of the Royal Artillery, who will proceed immediately to his station*.

As regards the proposed observatory at Ceylon, it has appeared to your Committee, on mature consideration, that Madras will be in many respects a preferable point, and they therefore have not insisted on Ceylon in their representations to Government, it being understood by them to be the intention of the Royal Society to recommend to the Honourable Court of Directors of the East India Company, to order the establishment of observatories in every respect similar, at that and two other stations in British India. This recommendation, your Committee understand, has been accordingly made, and at once acceded to in the most liberal manner, and the instruments immediately ordered; while Major Jervis, the provisional Surveyor-General of India, has recently visited Dublin for the purpose of familiarizing himself with the practical details and manipulations of the observations and instruments as adopted in the magnetical observatory of Dublin. Messrs. Riddell, Wilmot, and Lefroy, have also availed themselves of the same opportunity; and, by the subsequent erection at Woolwich of a set of the magnetic instruments, every facility has been afforded to the officers of the naval expedition for becoming acquainted with the processes.

The staff of each observatory will consist of the officer in charge, three non-commissioned officers, and two gunners, by which it is expected that the observations will be continued throughout the 24 hours; *i. e.* at every second hour throughout the day and night.

Each observatory is supplied with a very complete set of meteorological instruments, by which not only will the requisite data be afforded for the due correction and reduction of the magnetic observations, and for tracing the influence, if any, of meteorological changes on the magnetic elements, but a most valuable and complete series of meteorological observations will be procured at every station, made with compared instruments, and on a perfectly uniform system.

* Montreal has been subsequently changed for Toronto.

The naval expedition will be supplied with every instrument in duplicate in case of accident in the course of a long and difficult voyage, and to provide also for the contingency of the ships temporarily parting company.

Thus everything appears arranged so as to afford all human security for the attainment of the objects proposed in the most complete and satisfactory manner. And while your Committee feel themselves bound to acknowledge in strong terms the ample and liberal manner in which, on this important occasion, every demand on the national resources, without a single exception, has been granted, they consider it no less their duty to express their hopes that this splendid example will be followed up by other nations, and that this operation will thus become not merely a British, but an European and American one: that, in short, the opportunity thus afforded for combined and simultaneous exertion (such as the history of science has never yet offered) will be taken advantage of by observers, both public and private, in every region of the globe. The theory of terrestrial magnetism will thus at once be placed on a broad and ample basis of carefully observed facts, and the records of the next three years will be appealed to in every future stage of the progress of that science as its legitimate point of departure, in the new era which is opening for it.

Finally, as respects the application of the grant of 400*l.* made by this Association for the purchase of instruments, your Committee have to report that no part thereof has been expended, and they consider that none will be needed, as Government has charged itself with the entire expense of the instruments for the expedition, and for the observatories, under the direction of its own officers; and the Board of Directors of the East India Company have undertaken, with their usual liberality, their own observations at their own cost. The grant, however, has proved of most effectual service, as it has enabled your Committee, in more than one instance, to order instruments in the absence or in anticipation of distinct official authority to do so, and thereby to save much precious time, which, on the present occasion, has been hardly less valuable than money.

(Signed) J. F. W. Herschel.
H. Lloyd.

Report on British Fossil Reptiles. By Richard Owen, Esq., F.R.S. F.G.S. &c. &c.

1. THE British fossil organic remains referrible to the class Reptilia of Cuvier, if they do not indicate more numerous and diversified generic and specific forms, unquestionably exhibit more singular modifications of the typical structure of their class than do those belonging to any other primary group of the vertebrate division of animals.

The review which I have taken of the *Saurian* remains alone, which are treasured in different collections, has convinced me that they yield only to the Ichthyolites in the number of extinct species which they represent. And when it is remembered how large a proportion of the fossil fishes, described and figured in the classical work of M. Agassiz, includes species which are characteristic of the strata of Great Britain, it may be conceived that a report on our extinct reptiles could not be satisfactorily completed by me without the devotion of the leisure hours of more than a single year, nor be recorded in a brief space.

However captivating to the comparative anatomist such a subject must be from the rare and most singular conditions of organic structure manifested in the remains of these extinct and often highly developed cold-blooded animals, or however interesting from the important physiological relations traceable between their structural modifications and the conditions under which they once existed, and the parts assigned to them in the theatre of an ancient world,—nevertheless, I could not have ventured to have proposed to myself the 'British Fossil Reptilia' as a subject of continuous and systematic research, without the aid and encouragement which the British Association has liberally granted to me for that purpose.

Aware that the proposed report was not to be limited to a review of the actual state of the Reptilian branch of Palæontology,—a comparatively easy task,—but to embrace an account, founded, as far as might be, on actual observation, of those reptilian remains that have been hitherto discovered in different geological formations of the British Islands, I determined to divide the subject according to the natural families of the

class, and have selected the *Enaliosauria*, or 'Lizards of the Sea' —a race of which there is no longer any existing representative —for the subject of the present inquiry.

For the study of these remains I have visited the museums of the metropolis, of Cambridge, Birmingham, Bristol, Bath, Hull, York, Newcastle, Liverpool, Manchester, Lancaster, and other places, and have had access to the private collections of Viscount Cole, Sir P. Grey Egerton, Sir Astley Cooper, Dr. Johnson, Messrs. Hawkins, Bowerbank, Saull, T. Bell, and other gentlemen; to whom, as well as to the scientific curators of the public and provincial museums above cited, I beg to return my grateful acknowledgements for the liberal exposition of their fossil treasures, and their urbane attentions to every wish that arose out of my occupations.

As the comparison of the Saurian remains in these collections with those described or indicated in the recent treatises of Prof. Jäger, M. Hermann von Meyer, and other distinguished German Palæontologists, could not in all cases be made with satisfactory precision from the descriptions alone, I found it necessary to visit some of the principal depositories of the original specimens studied by those authors. At Frankfurt, besides the liberal access to the Senkenbergian museum, I enjoyed the privilege of examining the private collection and the valuable and extensive series of original drawings of fossils made by M. Hermann von Meyer, to whom I am particularly indebted for his attentions. I have much pleasure in making similar acknowledgements to Prof. Jäger of Stuttgard, and to Prof. Kaup of Darmstadt, the peculiar treasures of whose collection, however, belong to a higher class of *Vertebrata*.

The study of these collections has enabled me to identify some of the Saurians of the German lias beds with the species characterizing the corresponding strata in our own island, and, on the other hand, to obtain a certainty as to specific differences, which, without actual comparison, would have been only matter of conjecture.

Of the reptilian species, the fossil remains of which are the subject of the present report, the term of existence has long expired; and the peculiar modifications which characterized their type of structure, can now be studied only in the remains which the labours of the geologist bring to light. They will be here considered under three points of view, anatomically, zoologically and geologically: or, first, with reference to the restoration of the skeleton and the homology of its several parts to those of existing Vertebrates; secondly, as to the generic and specific modifications of the Enaliosaurian type, and the affini-

ties of the species; thirdly, with relation to the localities and extent of strata through which the several species are distributed, or to which they may be restricted.

Part 1. ENALIOSAURIA.—*General Characters of the Order.*

The *Enaliosaurs* were vertebrate, air-breathing and cold-blooded animals; referrible therefore to the great class of *Reptilia* in the Cuvierian system; and indicative of a primary modification of the typical structure of that class, by which they were fitted more especially for a marine life.

The proof that the *Enaliosaurs* respired atmospheric air immediately is afforded by the position and structure of the nasal passages, and by the osseous mechanism of the thoracic-abdominal cavity.

The evidence that they were cold-blooded animals, reposes on the unanchylosed condition of the elementary osseous pieces of the occiput and other cranial bones, of the lower jaw, and of the vertebral column: the laws of organic coexistence justify the conclusions from these conditions of the osseous system that the heart was adapted to transmit only a part of the circulating blood through the respiratory organs.

The peculiar modifications of the saurian type, or the special Enaliosaurian characteristics, consist in the absence of the ball and socket articulations of the bodies of the vertebræ; the position of the nostrils at or near the summit of the head; their separated hæmapophyses; the numerous, short, and flat digital bones, which must have been enveloped in a simple, undivided, tegumentary sheath, forming in both the fore and hind extremities a fin resembling in external appearance the paddles of the *Cetacea*.

Other genera of *Fossil Sauria* were aquatic and marine, and consequently possessed extremities modified for swimming, as are indeed those of the marine *Chelonia* of the present day, and in a less striking degree the feet of the Crocodiles among existing Sauria. But those reptiles only ought to be regarded as true Enaliosaurs which combine limbs fitted for swimming with the cranial and vertebral characters above defined*.

The *Enaliosauria* offer two principal modifications of their anatomical structure, of which the genera *Plesiosaurus* and

* The saurian system of M. H. v. Meyer, which includes the Teleosaurs and Mososaurs, with the true Enaliosaurs, on account of the modifications of the locomotive extremities, is not attended with any advantages compensatory of its extremely artificial nature. The bones of the extremities are less available than the vertebræ or teeth in indicating the generic and specific characters, or the true affinities of the individual Saurian to which they may have belonged.

Ichthyosaurus are the types; and I next proceed to consider the general characters of these two primary divisions of the order.

These characters are mainly derived from modifications of the vertebral column, as well with regard to the form and configuration of the individual bones, as to the relative proportion of the different groups of vertebræ called cervical, dorsal, caudal, &c. The vertebræ also frequently afford the best characters for the distinction of species, as well as of genera; and as they are the parts of the skeleton most commonly discovered in the strata characterized by the Enaliosaurian remains, they have received especial attention in the present Report.

To save much repetition, otherwise unavoidable in the subsequent pages, and to facilitate the comprehension of many of the descriptive details, it will be advantageous to premise some observations on vertebræ in general, before entering upon the modifications of these bones which characterize the *Plesiosauri* and *Ichthyosauri* respectively.

At the commencement of my examination of the fossil remains of the Enaliosaurians, I endeavoured to apply to the parts of the vertebræ, which in these animals are frequently complicated, and with the elements more or less dislocated, the views and nomenclature of M. Geoffroy St. Hilaire, whose analysis of a vertebra in the abstract has been generally adopted in this country. I was soon compelled, however, to relinquish the advantage which the vertebral theory of that philosophical anatomist seemed to promise; finding that it did not agree with my observations either on the cartilaginous or osseous centres as they appear in the development of a vertebra in the embryo; or on the fully-developed elements as they are exhibited in different classes of the vertebrate series, more especially in certain parts of the vertebral column of the *Plesiosaurus*.

I need hardly observe that a vertebra may be traced through its various degrees of complication, either during the progressive stages of its development, or by taking permanently-formed vertebræ of different grades of complexity in different animals; or, in many instances, by comparing the vertebræ in different parts of the spine in the same animal.

The terminal vertebræ of the tail in most species exhibit the simplest condition of these bones. The most complicated vertebræ which I have yet met with, are those at the lower part of the neck of certain birds, as the Pelican; or at the beginning of the tail of a Python, or other large serpent.

The parts or processes of such a vertebra may be divided into *autogenous*, or those which are independently developed in

separate cartilages, and *exogenous*, or those which shoot out as continuations from these independent constituents. The autogenous, or true elements, are,

1st. The *centrum*, or body of the vertebra, which, in Mammalia, as Cuvier has observed, is complicated by two *epiphyses*.

2nd. Two superior laminæ developed to protect the great nervous cord which rests on the upper surface of the *centrum*, and which I have therefore proposed to call *neurapophyses**.

3rd. Two inferior laminæ developed, generally to protect the great blood-vessels on the under surface of the centrum, and which I have proposed to call *hæmapophyses* †.

4th. The superior process‡ which is connected and generally anchylosed with the distal extremities of the *neurapophyses*, and forms, in conjunction with those processes, the superior arch of the vertebra.

5th. An inferior spinous process, which is connected, and commonly anchylosed with, the distal extremities of the *hæmapophyses*, forming, in conjunction with these, a chevron or V-shaped bone.

To the category of *autogenous* vertebral pieces belong the ribs, which generally are anchylosed to the other vertebral elements in the cervical, sacral, and caudal vertebræ of the warm-blooded vertebrate classes.

The propriety of regarding the ribs as vertebral elements is well illustrated, in the *Plesiosaurus*, in the cervical, sacral, and caudal vertebræ of which they have been generally described as transverse processes, although they are separate bones.

These elements bear the same relation to the centrum and its true transverse processes which the spinous processes do to the

* They are the *periaux*, or *perivertebral* elements, of Geoffroy St. Hilaire.

† These are the chevron-bones of Mr. Conybeare, the *paraaux* or *paravertebral* elements, of Geoffroy St. Hilaire; terms which he also applies to the costal processes, regarding these in the abdominal and thoracic regions as the expanded halves of the chevron-bones. If I had adopted Geoffroy's term, '*paraal*,' or its English equivalent, 'paravertebral element,' I must have diverted it from its original sense, in which it is applicable to two distinct elements, viz. the ribs and chevron-bones, which will be seen to co-exist in certain vertebræ of the *Enaliosauri*, and some existing animals; and I have preferred, therefore, to invent and define a new term, which has the advantage of expressing a physiological relation; and I am happy in being able to cite the authority of Cuvier for the propriety of this step. His words are, in reference to an analogous case, "Donner à un mot connu un sens nouveau est toujours un procédé dangereux, et, si l'on avait besoin d'exprimer une idée nouvelle, il voudrait mieux inventer un nouveau terme, que d'en detourner ainsi un ancien." *Mém. du Mus.*, tome ix. p. 123.

‡ This is regarded by Geoffroy, but without due grounds, to consist essentially of two lateral moieties, termed, *épiaux* or *epivertebral* elements.

neurapophyses and hæmapophyses, but they are more rarely anchylosed at their central or proximal extremities.

The length which the ribs sometimes attain need form no objection to their being regarded as parts of a vertebra, when it is remembered that the spinous processes, both above and below in some fishes, are longer than the longest ribs in the same skeleton. In the system of M. Geoffroy St. Hilaire, the nine elements of a vertebra are completed by reckoning the spines of the dermal skeleton, which in fishes are intercalated or articulated with the neural and hæmal spines of the true endo-skeleton as essential elements of a vertebra; and the *paraaux*, or *hæmapophyses*, are described as being developed in length and changed in direction, in order to form the vertebral ribs of the thoracic and abdominal regions.

The vertebræ of the Bird and Ophidian already alluded to, prove that vertebral ribs and inferior laminæ or hæmapophyses may co-exist; and the composition of the spine of the *Plesiosaurus*, especially in the caudal region, well illustrates this fact: for the costal appendages, which are generally anchylosed to the other vertebral elements, in the cervical, sacral, and caudal regions of the spine of the warm-blooded vertebrate classes, retain their original separate condition throughout the vertebral column in the *Plesiosaur*, and pass by such imperceptible gradations from one condition of physiological subserviency to another, that their nature cannot be mistaken when the entire series is studied in a complete skeleton; although, when seen in detached vertebræ of the neck or tail, they present the appearance, and have been generally described as, hatchet-bones, or transverse processes.

True transverse processes are, however, always *exogenous*, or mere projections from the centrum or the neurapophyses, and are of secondary importance. They are of two kinds, superior and inferior; both are present in the cervical vertebræ in most classes of the vertebrated animals; the inferior transverse processes alone are developed in fishes.

The *oblique*, or *articulating processes*, are also exogenous, and may be developed either from the neurapophyses, or the base of the superior spines of the vertebræ.

As in other complicated bones resulting from an association of several osseous pieces, certain elements of a vertebra may be modified in position and proportions, so as to perform the ordinary functions of others which may be atrophied or absent: thus in fishes, the inferior transverse processes are gradually bent downwards, until, in the dorsal region, their extremities meet and perform the functions of the hæmapophyses.

The costal processes or ribs are considered by Geoffroy St. Hilaire* to undergo in the *Cetacea* a similar change of direction, and also a dislocation from their usual attachments, and to have their distal extremities bent downwards and anchylosed to a rudimental spine, so as to assume the form and perform the offices of chevron-bones, or hæmapophyses ; but as the horizontal processes of the caudal vertebræ in the *Cetacea* (as exemplified in the skeleton of a young *Balæna antarctica*, fourteen feet in length, which I have lately had the opportunity of examining,) are originally developed from distinct centres, and in distinct cartilages, they appear to me to represent, with the corresponding separate vertebral elements in the Plesiosaurs, the true costal appendages of the tail, and the hæmapophyses must therefore be regarded as other and different elements of the vertebra. This view is supported by the fact that the long transverse processes supporting the ribs in the thoracic region of the spine in the same young whale, have no osseous nuclei developed in them, but are continuous cartilages from the still unossified parts of the centrum. I may observe also that the hæmapophyses in the young Cetaceans examined by me, exhibit what appear to be their permanent condition in the Enaliosaurians, viz. a want of bony union at their distal extremities ; at least I have never yet observed a true chevron-shaped bone, such as results from the anchylosis alluded to, in any skeleton of an Enaliosaurian.

Of the vertebral elements above enumerated the centrum is the most constant in its existence, but the neurapophyses and their spines are the most constant in regard to ossification : and there is an obvious reason, in the importance of the nervous cord which they are destined to protect, why these parts should be firm and resisting when circumstances might forbid the consolidation of the other vertebral elements. Thus, the neurapophyses are cartilaginous in the Lampreys, or *Petromyzontidæ*, while the centrum is gelatinous. The neurapophyses and their spines are completely ossified in the Lepidosiren, while the bodies of the vertebræ are represented by a fibro-gelatinous cord. A similar condition appears to have obtained in the fossil Microdonts and some other osseous fishes, in which the ossified neurapophyses and hæmapophyses have been preserved, while no trace of the bodies of the vertebræ remains.

CHARACTERS OF THE GENUS PLESIOSAURUS.

I now proceed to apply the foregoing views of the elementary parts of a vertebra, in the first place, to the exposition of the

* *Mém. du Muséum*, ix. p. 113.

generic characters of the vertebral column in the *Plesiosauri.*

The most conspicuous and remarkable feature of this part of the skeleton, is the extraordinary length of its cervical portion, which includes from twenty to forty vertebræ. The articular surfaces of the bodies of the vertebræ are either flat or slightly concave, or most frequently convex in the centre and concave at the periphery. In general the bodies present two pits at their under part, but this character is not constant.

The cervical vertebræ of the *Plesiosauri* generally present the following parts in a separate or unanchylosed state,—the centrum, the neurapophyses, and ribs; and it is interesting to observe that although, in general, no transverse processes are developed in this region, an analogy with the structure characteristic of this part of the spine in the Crocodile is maintained in the division of the articular surface for the cervical rib into an upper and lower portion by a horizontal fissure; which structure is well described and figured by Mr. Conybeare in the *Plesiosaurus dolichodeirus.*

In Mammalia, the interspace of the two cervical transverse processes on each side is occupied by the vertebral artery: in Birds, by the vertebral artery and sympathetic nerve: in the *Plesiosaurus* it is too inconsiderable to lead us to imagine it to have been subservient to the protection of any important vessel or nerve, but its existence, besides being referrible to the law of adherence to type, may also have had relation to the presence of an interarticular ligament for the purpose of connecting the head of the cervical rib or hatchet-bone to the body.

As the cervical vertebræ in the genus *Plesiosaurus* approach the dorsal, the inferior part of the costal articulation becomes smaller, and a corresponding increase of surface is afforded by the superior facet, which also gradually rises from the centrum to the neurapophyses, and in the dorsal vertebræ stands boldly out as a true transverse process from the upper side of the base of each neurapophysis.

At the sacral vertebræ, however, the transverse processes subside to the level of the neurapophyses; and as the caudal vertebræ recede from the trunk, the articular surface, which, as in the neck, represents, or is in the situation of, the transverse process, gradually descends, and passes from the neurapophysis to the side of the centrum; but it is not divided by the longitudinal groove which characterizes the costal surface in the neck.

This groove is more marked in some than in other species of *Plesiosaurus;* and I have seen Plesiosaurian vertebræ undoubtedly cervical, in which no trace of it was visible.

The neurapophyses are commonly unanchylosed with the ver-

tebral centres in any part of the spine, and in some instances throughout the cervical, and at the anterior part of the dorsal region, the neurapophyses have appeared to be joined each by an articular surface to the spine above, as they are to the centrum below,—the spines here remaining apparently throughout life unanchylosed to the neurapophyses. This condition of the upper vertebral elements is rarely seen in any cold-blooded vertebrate, and never in the warm-blooded classes.

In those parts of the spine where the vertebræ enjoyed less mobility upon each other than in the neck, the spines become anchylosed to the neurapophyses at an earlier period.

The hæmapophyses co-exist with the ribs or paravertebral elements in the caudal region of the spine, but they continue throughout life to be unattached by bone either to the centrum above or to each other below; and here also their spine is not developed, and consequently no true chevron-bone is formed in the *Plesiosauri*. The hæmapophyses are also continued along the inferior surface of great part of the abdomen, forming there the sternal or abdominal ribs; and just as the neurapophyses are developed in the transverse direction to protect the expanded cerebral masses in the cranial region, so here the hæmapophyses are in like manner elongated transversely, and their spine is introduced and modified to form a third mesial rib-like bar, connecting, however, as usual, the lower or distal extremities of the hæmapophyses, and completing the osseous cincture of the abdominal viscera.

The tail in the *Plesiosauri* is relatively much shorter than in the *Ichthyosauri*, and there is an obvious reason for the curtailment of this part of the animal; for in the *Plesiosauri*, the length and mobility of the neck renders a special development of the tail for producing the lateral movements of the head unnecessary.

The bodies of the vertebræ, in most species of *Plesiosaurus*, are traversed vertically by two vascular canals, which lead from the medullary or spinal canal to the inferior surface of the centrum, where they terminate each by an orifice, and sometimes by two orifices, on each side the middle line. These orifices are not, however, a constant character of the genus *Plesiosaurus*, neither are they peculiar to this genus, being present in the vertebræ of the *Cetacea*, as well as in those of other *Sauria*.

In a section of the vertebral centrum of a *Plesiosaurus*, the osseous texture for some lines near the anterior and posterior articular surfaces is denser than in the rest of the vertebræ, and the direction of the laminæ and fibres is vertical: in the intermediate portion the laminæ are horizontal.

The head of the *Plesiosauri* resembles that of the Crocodiles in its general form, but is relatively much smaller in proportion to the body: the elongated form of the strong and prominent cranial bones, most of which extend from point to point, with wide interspaces like the timbers of a scaffolding, forms one of the first indications of a deviation from the Crocodilian type, and of the affinity of the *Plesiosaurus* to the Lacertian *Sauria;* and this affinity is further exemplified in the condition of many of the individual bones.

The occipital bone includes the basilar, lateral or ex-occipital and supra-occipital pieces in a permanently separated condition, as in other Reptiles. The basi-occipital forms a larger proportion of the articular tubercle for the atlas, and the ex-occipitals a less proportion, than in the Crocodiles; and the circumference of the *foramen magnum* is completed by the supra-occipital element; in both which respects the *Plesiosaurus* manifests its affinity with the Lacertian *Sauria.*

The mastoid elements extend from the occipital to the tympanic bones; but above these and between the occiput and the strong arched pedicle supporting the lower jaw there is a vacuity leading from the occipital region into the temporal fossæ. The corresponding openings in the skull of the Crocodiles are reduced to very small size in consequence of the expanded form and oblique position of the tympanic bone, but in the Lacertian *Sauria* they are as wide as, if not wider than, in the *Plesiosaurus*.

The parietal is a strong triradiate bone in the *Plesiosaurus*, consisting of a median piece corresponding with the normal parietal in the Crocodiles, and of two transverse elongated processes, formed, as it were, by a bifurcation of the posterior part of the median piece.

In a young specimen of *Plesiosaurus macrocephalus* in the collection of Viscount Cole, the median or sagittal suture dividing the two parietals is still distinct: in older specimens of the *Pl. Hawkinsii* I have always found it obliterated, so as to justify the above description of the parietal as a single triradiate bone.

The median portion of the parietal offers two modifications of structure which point out in a striking manner the deviation of the *Plesiosaurus* from the Crocodilian, and its approximation to the Lacertian type of the Saurian structure.

The first of these characters is the median crest or ridge from which the surface slopes away on each side; proving that the temporal muscles were relatively as strongly developed as in the *Iguanæ*, *e. g.*, and were only separated from one another at the top of the head by the intermuscular ridge. In the Crocodiles on the contrary, in which the ponderous jaws are worked

principally by the masseteric and pterygoid muscles, the temporals are small, and are separated from each other by a flattened space occupying nearly the whole of the simple parietal bone.

The second character of the median part of the parietal, which brings the *Plesiosaurus* near to the Lizard tribe, is a moderate-sized elliptical vertical perforation of the bone, a few lines behind the coronal suture, which perforation is analogous to that described by the Rev. Lansdowne Guilding in the *Iguana* under the name of the *Foramen Homianum*, where, however, it is situated directly upon the coronal suture, in the situation of the *anterior fontanelle*. The same foramen, however, exists in many other genera of Lacertian *Sauria*; and in *Monitor*, *Lacerta* proper, &c. it is situated, as in the *Plesiosaurus*, entirely in the parietal bone. There is no trace of this foramen in the Crocodilian *Sauria*. The posterior bifurcation of the parietal bone forms a third instance of the resemblance of the *Plesiosaurus* with the Lacertian, and its difference from the Crocodilian structure. These processes are of considerable strength, and commonly form the most prominent parts of the cranium in fossil specimens: they articulate by means of an oblique sigmoid suture with the tympanic bone*.

Frontal.—The frontal bone consists of a median, two anterior and two posterior pieces. The median frontals extend as far forwards as the midspace between the small nostrils, and appear to terminate in a point between the commencement of the narrow nasal bones. The interfrontal suture in the young *Pl. macrocephalus* before alluded to, is elevated by a ridge continued forwards from the parietal crest. The outer margin of the median frontal forms the superior boundary of the orbit. The anterior frontal enters into the formation of the anterior and superior angle of the orbit, and is wedged in between the mid-frontal and superior maxillary bones. The posterior frontal bounds the orbit posteriorly, and extends downwards to join the malar bone, like the columnar portion of the post-frontal bone in the Crocodiles; but it is broader and more superficially situated in the *Plesiosaurus*, and thus resembles more the corresponding part of the cranial structure in the Lacertian *Sauria*. The posterior frontal differs further and in a more striking degree from the Crocodilian type in not being extended backwards to join the mastoid; so that the skull of the *Plesiosaurus* does not present,

* One of Miss Philpott's specimens exhibits the parietal of a *Pl. dolichodeirus* thinned off posteriorly, and rugous, apparently forming an articulating or sutural surface for the overlapping of the tympanic bone.

In most specimens the sagittal suture, dividing the median parietals, is persistent.

as in the Crocodiles, an osseous ridge traversing longitudinally the temporal fossa, like a second zygoma, and dividing it into an upper and a lower cavity.

Zygomatic.—The zygomatic element of the temporal bone, instead of being extended obliquely, parallel with, and joined to the tympanic bone, stretches horizontally from the malar and post-frontal backwards to the lower end of the tympanic, and is attached, as in the Lacertian *Sauria*, only by its two extremities.

Tympanic.—The tympanic bone in its general form, and especially its length, is intermediate to the Crocodilian and Lacertian types, but exceeds them both in its robustness.

Facial bones.—When we come to examine the bones of the face, the resemblance to the Lacertian *Sauria* begins to diminish, and that to the Crocodiles to increase. This tendency to the higher types of Saurian organization is shown in the strength of the whole maxillary apparatus, in the great relative size of the intermaxillaries, the rugged exterior surface of the bones, and above all in the distinct alveolar cavities for the teeth.

The external nostrils, however, form a striking exception to this tendency; and their size and position, combined with the structure of the paddles, indicate the analogy of the extinct Enaliosaurs to the existing Cetaceans, and offer a beautiful example of the adaptation of structure to the peculiar exigencies of a species.

The apertures through which the air is respired are situated a little anterior to the orbits near the highest part of the head. In the Crocodiles they are situated, as is well known, near the anterior extremity of the snout, are blended together into a single aperture, and nearly the whole of their circumference is formed by the intermaxillary bones. In the *Plesiosaurus* the intermaxillaries form an extremely small part of the boundary of the nasal apertures, which chiefly consist on each side of an interspace at the convergence of the anterior frontal, nasal, and superior maxillary bones; the nostrils are also separated from each other by the nasal bones, as in the Lacertian *Sauria*.

The intermaxillary suture extends from the anterior part of the nostrils forwards to a little more than halfway between the orbit and the anterior extremity of the cranium. One of the strongest of the inferior teeth usually rises just in front of this suture, and a slight notch at that part seems to correspond with that tooth, presenting a resemblance to a very characteristic structuse in the true Crocodiles.

The lachrymal bone forms a great proportion of the anterior part of the orbit: the superior maxillary enters next into the

formation of the circumference of the orbit below the lachrymal; and the malar bone rests by an oblique suture upon its posterior extremity. The posterior margin of the malar bone is joined to the posterior frontal as well as to the zygomatic bone, and thus completes the osseous boundary of the orbit posteriorly.

Lower jaw.—The lower jaw of the Plesiosaur presents the complicated structure usual in the Saurian order. The dentary piece appears soon to become anchylosed to its fellow at the symphysis, and is chiefly remarkable for the expansion of its anterior extremity. The angular and surangular pieces are not separated by an intervening vacuity as in the Crocodiles, but are joined together throughout as in the Lacertian group. The surangular rises higher and forms a sharper edge for the insertion of the temporal muscles than in the Crocodiles, a structure which agrees with the greater development of these muscles, as indicated by the size of the temporal fossæ. The articular piece presents a regular and deep concavity for the reception of the articular end of the tympanic bone: it is, as Mr. Conybeare has well remarked*, more developed than in the Crocodile, and thus approximates more nearly to the corresponding part in the Lacertian type. The angular piece is prolonged backwards beyond the joint, but not quite to the same extent as in the Crocodiles.

Sterno-costal arcs.—The ordinary or vertebral ribs have been already spoken of as essential parts or appendages of a vertebra: their free extremities are connected together, in the abdominal region, by a series of intermediate slender elongated pieces, termed by Mr. Conybeare the 'sterno-costal arcs.' Each arc includes, in the *Plesiosaurus*, seven pieces: the median one is transversely elongated, slightly bent, and pointed at both extremities; the lateral pieces have a similar form, except that the extremity of the outermost, which joins the vertebral rib, is obtuse: each piece as it recedes from the median line overlaps the anterior part of the one which it succeeds, where it is adapted to an oblique groove. This kind of joint probably admitted of a yielding or sliding motion of the pieces one upon the other, and favoured, as Dr. Buckland has observed, considerable expansion of the cavity containing the lungs.

Pectoral arch.—Of the bones composing the pectoral arch the broad coracoids are the most conspicuous on account of their remarkable expanse in the antero-posterior direction; their internal and anterior margins are gently convex, and meet at the mesial plane, where they overlap the anterior thoracic ribs. The ento-sternal piece is wedged into their anterior interspace;

* *Geol. Trans.*, 1822, p. 121.

it consists of a short mesial process, and two broad lateral expansions.

A strong triradiate bone, which seems to represent, as in the *Chelonia*, the scapula and clavicle united, arches from the outer extremity of the ento-sternal branch to the corresponding extremity of the coracoids, with which it combines to form the shoulder-joint, and near which point it sends upwards and obliquely backwards a branch or process representing the true scapula.

Pectoral extremity.—The humerus is a stout and moderately long bone, rounded at its proximal extremity, and flattened as it approaches the elbow-joint: it is curved slightly backwards.

The radius and ulna are both short and flat bones, but relatively longer and more distinctly marked than in the *Ichthyosauri*: the radius or anterior of the two bones is nearly straight; the ulna is curved, with its concavity directed towards the radius.

The carpus is very distinctly defined, consisting of a double row of small flat rounded ossicles, from six to eight in number. The metacarpal bones, five in number, are elongated, slender, flattened, and slightly bent. The digits never exceed the number of the metacarpal bones, but consist generally of more than the usual number of phalanges. The first, or radial one, corresponding with the thumb, has generally 3, the second 6 or 7, the third 8 or 9, the fourth 8, and the fifth 6 phalanges. These bones are moderately long and slender, but gradually taper towards the distal end of the digits: they are joined together in each digit by flattened surfaces, indicative of a mere yielding movement on one another. There can be little doubt that they were enveloped, like the paddles of the *Cetacea*, in a common sheath of integument. From the natural curve of the digits, the paddles of the Plesiosaur must have had a more elegant and tapering form, and have possessed greater flexibility, than those of the modern *Cetacea*.

Pelvic arch.—The hinder or pelvic extremities almost always equal, and sometimes, as in *Pl. macrocephalus*, exceed the anterior ones in size.

The pelvic arch consists of a short and strong ilium, and a broad pubis and ischium, both of which are expanded in the antero-posterior direction, analogously to the coracoids in the pectoral arch.

Pelvic extremity.—The radiated appendages of the pelvic arch so closely correspond with those of the pectoral arch as to require little to be said respecting them. In the modifications of the two bones of the leg, the posterior one, or fibula, corresponds in its curved form with the ulna, and illustrates an

analogy which is indicated in other animals. The tarsal bones are principally remarkable for their small size on the tibial or anterior side of the series, indicating that the hind paddle had a freer inflection forwards, or upon the tibia, than in the opposite direction. This structure may have given a compound motion to the propelling stroke of the paddle, similar to that which in skilful rowing is called 'feathering' the oar.

The five metatarsals and their digits correspond in structure with those of the fore paddle. The first or tibial metatarsal supports 3 phalanges, the second 5, the third 8 or 9, the fourth 8, and the fifth 6 phalanges. The articular extremities of the phalanges of both the fore and hind paddles are subconcave, with an irregular surface, indicating that they were joined by ligaments or fibro-cartilage, and not by synovial membrane.

Plesiosaurus Hawkinsii.

Having now given a general sketch of the skeleton of the *Plesiosaurus*, I proceed next to point out the modifications of this type in the different species; and I shall begin with that of which the greatest number of complete or nearly complete skeletons exist in the British Museum and other collections.

The species to which these skeletons belong is described and figured in Mr. Hawkins's memoir on *Ichthyosauri* and *Plesiosauri* under the name of *triatarsostinus*; but as this designation relates to an imperfect state of the tarsus in the right foot, (for a fourth bone is present in the left tarsus of the same specimen, and a second specimen of the same species in Mr. Hawkins's collection exhibits five tarsal bones on each side,) I propose to describe it under the name of Hawkins's Plesiosaur (*Plesiosaurus Hawkinsii*) as a sincerely offered though inadequate tribute of admiration of the indefatigable labour and rare skill with which its remains have been disencumbered of their earthy shroud.

The head of the *Pl. Hawkinsii* is of moderate size, smaller in proportion than in the *Pl. macrocephalus*, and somewhat larger than in the *Pl. dolichodeirus*. The neck equals three lengths of the head, and the neck and head together equal the trunk and tail. The number of vertebræ throughout the spinal column is between 90 and 100. In the first or anterior 31 vertebræ the centrum supports the whole or part of the costal pit: from the 32nd to the 56th vertebra inclusive the costal articular surface is wholly impressed upon the neurapophysis: from the 55th vertebra the costal pit begins again to descend upon the side of the centrum, and it has entirely left the neurapophysis at the 61st vertebra. At the 80th vertebra the costal processes disappear.

In consequence of the unequivocal presence of ribs throughout so great a proportion of the vertebral column, the ordinary characteristics of cervical, dorsal, lumbar, and caudal vertebræ are wanting, and a definition founded on the relative size or form of the costal elements becomes, from the gradual manner in which they alter in these respects, very ambiguous and difficult in its application. I have therefore proposed, in order to gain a surer point of comparison of the different species of *Plesiosauri*, to reckon those vertebræ as *cervical* in which the centrum exhibits the whole or a part of the costal articular surface. The body of a cervical may always be distinguished from that of a caudal vertebra in being without any trace of hæmapophysial pits. The *dorsal* vertebræ are those in which the costal surface is situated wholly on the neurapophysis. The *caudal* vertebræ are characterized by having both costal and hæmapophysial impressions on the body, except the terminal ones, which are readily distinguished by their small size, the absence of both the above-named impressions, and by the concave character of the articular surfaces of the bodies.

The cervical vertebræ present the following characters in the species under consideration: taking the transverse diameter of the body of the vertebra at 10, the vertical diameter of the same is 9, and the antero-posterior 8. The articular surfaces present the normal Plesiosaurian character, being slightly concave, with a gentle convex rising in the centre of the concavity.

The exposed or nonarticular surface of these vertebræ is smooth. The costal *pit* is longitudinally elliptical, situated near the lower part of the centrum in the anterior two thirds of the cervical region, and having a space equal to its vertical diameter intervening between it and the lower extremity of the neurapophysis: and here I may observe that the character afforded by the relative extent of this space is a very useful one, as it is variable in the species but constant in each; and as it is indicated by the centrum alone, it serves as a term of comparison when the other elements of the vertebra may be lost. The articular base of the neurapophysis is bounded below by two lines meeting at an open angle. From the apex of this angle to the articular process the distance is less than the extent of the centrum below the apex. The spines are compressed throughout, slightly curved backwards, with the anterior angle and apex rounded off. This character is gradually changed at the base of the neck for a quadrate form of the spine with a straight truncate apex; and towards the posterior part of the dorsal region this apex is slightly thickened.

The height of the spine is to its antero-posterior diameter as

8 to 5 through the greater part of the dorsal region. The tail includes $2\frac{1}{2}$ lengths of the head, and from the posterior end of the ischia consists of 35 vertebræ. The same general form and proportions of the vertebræ are preserved throughout the vertebral column; and it is this fact, established upon a comparison of four nearly entire specimens in the collections of the British Museum and of Mr. Hawkins, which enables me to speak with confidence of the specific importance of well-marked characters, though they may be afforded by detached vertebræ only.

I have few observations to offer on the specific peculiarities of the head of the *Pl. Hawkinsii*, as it is principally from the perfect specimens of this species that the description of this part in the general account of the *Plesiosauri* has been taken. Its specific character will be manifest when I come to compare with it the head of the *Pl. macrocephalus*. I may remark here, however, that the orbit occupies a position halfway between the occipital condyle and the end of the snout. The bones have a smooth surface, except at the anterior part of the head, where there are many pits and grooves, like those in the head of the Crocodile. The teeth participate in the general character of the Plesiosaurian type, being long, slender, slightly recurved, finely but distinctly grooved in the longitudinal direction on the outer surface, with a long pulp-cavity within. There are about 40 teeth on each side of the upper and 35 on each side of the lower jaw: those towards the anterior extremities of the jaws are longer than the rest, but the disproportion is more strongly marked in other species than in the present.

Extremities.—The fore and hind extremities of this species are nearly equal in size, but the latter are a little longer.

The pectoral arch accords with the general Plesiosaurian type.

The sternum has no median process; it presents a well-marked concavity anteriorly.

The anchylosed scapula and clavicle form a triradiate bone, of which the scapular portion is short and compressed, directed obliquely backwards at an angle of 45° with the clavicular part, and equalling two thirds of the extent of this bone.

The coracoid equals in antero-posterior extent eight of the cervical vertebræ immediately anterior to it.

The humerus equals in length $6\frac{1}{2}$ of the posterior cervical vertebræ: its anterior margin is slightly convex: the breadth of its distal flattened extremity equals twice its length.

The radius is longer than the ulna, the breadth of which does not quite equal its length.

Six appears to be the normal number of carpal bones.

In the hinder extremity the bones of the pelvic arch first claim attention. The ileum is flattened and slightly expanded at its superior free extremity, where it rests upon the sacral ribs; its length is equal to four of the contiguous vertebræ. The pubic bones are in the form of large square-shaped plates, with the angles rounded off, and a deep smooth emargination at their posterior edge, which is turned towards the corresponding emargination of the ischium to form the 'foramen ovale.' The outer and posterior angle is marked by the articular surface contributed by the pubis to the formation of the acetabulum. The antero-posterior extent of the broad pubic bone equals in this species nearly four of the parallel vertebræ.

The ischia present the form of inequilateral triangles, and are straighter at their mesial edges than are the pubic bones: they present a concavity at each of the other margins; which is deepest at the shorter and anterior margin. The length of the ileum exceeds that of the pubis. The extent through which the median margins of the ischia are joined to each other exceeds that in other species of Plesiosaur.

The femur is more slender in the shaft than the humerus, but is of the same length: its distal flattened extremity is less expanded.

The tibia and fibula are more nearly equal in length than are the corresponding bones of the fore paddle; the breadth of the reniform fibula equals its length. There are at least five bones in the tarsus of this species. That which is wedged into the interspace between the distal extremities of the tibia and fibula is characterized by a concave notch at its tibial margin. The number of digital phalanges, in addition to the metatarsal row, corresponds with that given in the description of the generic type.

Full-grown individuals of this species appear to vary from 7 to 7½ feet in length.

Localities.—The *Plesiosaurus Hawkinsii* is most common in the lias quarries near Street. It occurs at Lyme; but is less common there than the *Pl. dolichodeirus* and *Pl. macrocephalus* next to be noticed.

Vertebræ of this species have been found in the lias at Weston near Bath; in the lias bone-bed at Aust-Cliff in the neighbourhood of Bristol, and in that of the Pyrton passage on the Severn. I have not yet seen any specimens referrible to this species from the lias of Whitby or from that of Boll in Wirtemberg.

Plesiosaurus dolichodeirus.

The admirable description and restoration of this species

given by Mr. Conybeare in the second part of the first volume of the second series of the *Geological Transactions* leaves little to be said excepting in regard to those points in which it differs from the *Pl. Hawkinsii.*

The head is relatively smaller in proportion to the body; forming less than the thirteenth part of the whole length of the skeleton, while in the *Pl. Hawkinsii* it forms less than one tenth part.

This diminutive head was supported on a longer neck. In the *Pl. Hawkinsii* the head is three times the length of the neck; while in the *Pl. dolichodeirus* it is four times that length. Mr. Conybeare states that the neck of the *Pl. dolichodeirus* is fully equal in length to the body and tail united; but in Hawkins's Plesiosaur the length of the neck only slightly exceeds that of the body or trunk; and this difference depends both on a difference in the number as well as in the form of the cervical vertebræ.

The cervical vertebræ in the *Pl. dolichodeirus*, reckoning as such those which supported hatchet-shaped, and not rib-like, lateral appendages, are, according to Mr. Conybeare, thirty-five in number; while the corresponding vertebræ in Hawkins's Plesiosaur are twenty-nine in number. The cervical vertebræ in the latter are also shorter in proportion to their breadth than are those of the *Pl. dolichodeirus.*

The dentary bone has a shorter and less expanded symphysial portion, and the anterior teeth have a smaller proportional size than in the *Pl. Hawkinsii* or *Pl. macrocephalus.*

A lower jaw of this species in the collection Miss Philpotts, measures

	In.	Lin.
in length	5	8
in breadth behind the teeth	3	6

The number of teeth in this lower jaw was 50 (25–25).

The spinous processes of the vertebræ are more compressed laterally in the *Pl. dolichodeirus* than in any other species of *Plesiosaurus* which I have seen.

A more readily appreciable difference is presented in the forms and relative sizes of the ulna and tibia in these nearly allied species. In the *Pl. dolichodeirus*, the ulna, or posterior of the two bones which succeed the humerus, is as long as the radius; and its margin next the radius is but slightly concave. In Hawkins's Plesiosaur the ulna is shorter than the radius, broader in proportion to its length, and with a deeper concavity on its inner margin.

In Hawkins's Plesiosaur, the fibula, in regard to its relative

length and breadth, and its bent or reniform figure, and particularly with respect to the curvature of its outer margin, deviates in a greater degree than the ulna from the corresponding bone in the *Pl. dolichodeirus*. The differential characters afforded by the bones of the fore arm and leg are the more satisfactory, because, as we shall presently see, the *Pl. macrocephalus* again presents different and characteristic forms of the same bones. There are other and slighter differences in the shape of the hatchet-bones, or cervical ribs, of the humerus and of the femur.

The length of the skeleton described by Mr. Conybeare is nearly ten feet.

Localities.—The most common places of deposit of the bones of this species are in the lias of Somersetshire at Watchett, Bath and Bristol; and in that of the valley of Lyme in Dorsetshire. I have likewise seen detached vertebræ of the *Pl. dolichodeirus* from the lias of Bitton in Gloucestershire.

Plesiosaurus macrocephalus.

The characters of this interesting species I have fortunately been able to study, not only in detached bones in different collections, but also in an almost entire specimen liberally placed at my disposal for that purpose by Viscount Cole.

As, however, only a portion of the tail is preserved in this unique specimen, the total number of vertebræ characteristic of the *Pl. macrocephalus* still remains to be ascertained.

The cervical region of the spine in this species exhibits the prominent character of the genus in its great extension. It is, however, only twice the length of the lower jaw, instead of three times the length of the same part, as in the *Pl. Hawkinsii*; and this difference, arising from the greater development of the head in the *Pl. macrocephalus* is associated, as Dr. Buckland has observed, with thicker and stronger vertebræ in relation to the greater weight they had to sustain. It includes twenty-nine cervical vertebræ. In the twentieth cervical vertebra of *Pl. Hawkinsii*, the transverse is to the antero-posterior diameter as 4 to 3. In the corresponding vertebra of the *Pl. macrocephalus* the transverse is to the antero-posterior diameter very nearly as 2 to 1. The rest of the cervical vertebræ bear a similar ratio to those of the *Pl. Hawkinsii*; the bodies of the vertebræ therefore in *Pl. macrocephalus*, although by no means so flat as in the *Ichthyosauri*, make an evident approach to the characteristic form of the vertebræ in that genus.

In the *Pl. Hawkinsii* the hatchet-shaped processes are converted into styliform ribs at the twenty-ninth cervical vertebra;

but in the *Pl. macrocephalus* they undergo this change of form at the twenty-seventh cervical vertebra, and perhaps at the twenty-fifth; but this appendage is lost in the skeleton under consideration.

Hence we may conclude that the *Pl. macrocephalus* has two vertebræ less in the cervical region than the *Pl. Hawkinsii*, and probably six cervical vertebræ less than the *Pl. dolichodeirus*, in which Mr. Conybeare states that "the thirty-five anterior vertebræ exhibit these (hatchet) processes distinctly characterized, and are therefore, beyond all doubt, cervical*."

The articular surfaces for the ribs on the anterior cervical vertebræ of the *Pl. macrocephalus* are relatively larger and have a more regular lozenge-shape than in the *Pl. Hawkinsii*, in which they are elongated in the axis of the vertebra. They are traversed (as mentioned in the general characters of the Plesiosaurian vertebræ,) by a longitudinal groove; this gradually sinks from the middle of the depression towards its lower margin, and at length, at the twenty-third cervical vertebra, disappears.

The depressions above the costal surfaces for the lodgement of the bases of the neurapophyses resemble in form those of the *Pl. Hawkinsii*, but extend further down upon the side of the centrum. They are co-extensive with the antero-posterior diameter of the vertebral body, and are bounded by two lines meeting below at a right angle. The angle formed by the corresponding lines in the *Pl. Hawkinsii* is more open. The distance between these neurapophysial pits and the costal pits in the anterior cervical vertebræ, differs in different species of *Plesiosaurus*. In the *Pl. macrocephalus* the interspace is very short, never exceeding half the diameter of the costal pit, even in the most anterior of the costal vertebræ. In the *Pl. Hawkinsii* the interspace is equal to double the diameter of the costal pit in the corresponding vertebræ.

There may also be observed in the *Pl. macrocephalus* an evident tendency in the surface supporting the cervical vertebræ to rise above the level of the centrum; and this is the more interesting as in a large species of *Plesiosaurus* allied to *Pl. macrocephalus* (the *Pl. arcuatus* subsequently to be characterized) the surface on the centrum, and the corresponding surface of the neurapophysis do project as short transverse processes, and thus approximate to the Crocodilian type.

As the seventh, eighth, and ninth vertebræ happen to be displaced in Lord Cole's specimen, and their neurapophyses to be dislocated, the form and breadth of the articular depressions for

* *Loc. cit.*, p. 384.

the neurapophyses, as well as the canal for the spinal marrow, are thus brought into view. The centrum presents only a plane surface for the spinal cord, the rest of the canal being completed by the neurapophyses laterally, and the expanded base of the spine above. The surface in question is bounded by two lateral curved lines, having their convexities turned upwards towards each other. Immediately below, and external to this surface on each side, are the deep and roughened pits for the attachment of the neurapophyses.

The cervical neurapophyses do not in any of the Plesiosaurs unite immediately together above the spinal cord and canal, so as to form a continuous bony arch, spanning across that part; but they stand upright from their sockets in the vertebral body, parallel with each other, or only slightly converging at their superior extremities. They terminate above, in young individuals at least, in broad rough articular surfaces parallel with the transverse axis of the vertebræ, but sloping down from behind forwards with a slight sigmoid flexure at an angle of 25° with the longitudinal axis of the vertebra.

In the same way, therefore, as the rib, or appendage to the transverse processes, is bifurcate at its proximal extremity, in those cases where the two transverse processes are separately developed on each side of the vertebra, and where the rib is joined to both; so here the spinal appendage of the neurapophyses is bifurcate at its proximal extremity, and each fork rests upon the above-described oblique articular process of its own side.

This analogy between the lateral or costal, and the superior appendages of the vertebral centre, is one which the *Plesiosaurus* alone has hitherto afforded.

But besides the two surfaces developed for these articulations with the neurapophysis, each fork of the spine sends off an articular or oblique process from its anterior and posterior extremity; the articular surface looking obliquely upwards and inwards on the anterior process, and downwards and backwards on the posterior process: and thus the spines are locked together throughout the whole vertebral column with the exception of the terminal vertebræ of the tail.

In adult individuals of the *Pl. macrocephalus*, these separate elements of the superior arch become anchylosed together, as is the case in a great part of the spine in the present specimen.

In a Plesiosaurian cervical vertebra, however, measuring seven inches and a half in vertical extent and three inches and a half in transverse diameter, in the collection of Mr. Hawkins, I find the neurapophyses distinct both from the spine

above and the centrum below. But in other cervical vertebræ of a still larger Plesiosaur in the collection of Lord Cole, not only is the spine anchylosed with the neurapophyses, but these are also confluent with the centrum.

In the dorsal region in the *Pl. macrocephalus*, as in the *Pl. Hawkinsii* and *Pl. dolichodeirus*, the neurapophyses and spines become anchylosed; but the former elements continue separate from the body of the vertebra throughout the vertebral column in the *Pl. macrocephalus*.

The cervical spines in the *Pl. macrocephalus* differ in form from those of *Pl. Hawkinsii* in retaining their breadth or antero-posterior extent throughout the neck; their extremities being, as it were, truncate, with the angles slightly rounded off. The powerful ridge of bone which they thus collectively form is highly characteristic of this species. The consequence of this structure is a diminution of the spinal interspaces necessary for the vertical inflections of the neck; which interspaces are conspicuously present in the *Pl. Hawkinsii*, where the end of each cervical spine is as it were obliquely cut off at the anterior part, so as to allow the neck to be bent upwards much more extensively than could have been possible in the *Pl. macrocephalus*. What, however, the latter species thus lost in mobility it gained in strength, the quality mainly required in relation to the movements of its more bulky and ponderous head and jaws.

As the powerful neck of the *Pl. macrocephalus*, however, possessed extensive mobility in the lateral direction, as is indicated by its position in Lord Cole's fossil, the muscles destined for these movements must necessarily have been developed in a corresponding degree; and we find that adequate provision was made for their fixed points of action, in the superior development of the costal processes, as compared with those of *Pl. Hawkinsii*: these processes present, indeed, throughout a greater part of the neck the characteristic expansion of their distal extremities, which led to their being called hatchet-shaped bones by Mr. Conybeare: but the stem which supports the dilated extremity is proportionally longer in the *Pl. macrocephalus*; and it is only towards the base of the neck that the extremities overlap each other, as in the Crocodile. Dr. Buckland has illustrated this peculiarity by placing side by side the figures of the hatchet bones in the *Pl. Hawkinsii* and *Pl. macrocephalus* in his Bridgewater Treatise. These cervical ribs assume the true costal form, as before stated, at the twenty-seventh vertebra, where they are short and straight; behind this

part they progressively increase in length, and become bent towards the sternal region.

The cervical vertebræ gradually increase in all their dimensions (least so however in their antero-posterior extent) as they approach the trunk; but the difference in their size at the two extremities of the neck is less than in the *Pl. dolichodeirus*.

Dorsal Vertebræ.—These are characterized, according to the previous definition, by the absence of articular surfaces on the centrum for ribs, and by the development of a superior transverse process, which exclusively supports the rib, from the base of each neurapophysis. The number of vertebræ so characterized is twenty; that of the corresponding vertebræ in the *Pl. Hawkinsii* is twenty-three.

These vertebræ include, besides the ordinary dorsal, those which occupy the situation of the lumbar or ribless vertebræ in the Crocodile; but there are no such vertebræ in the trunk of the Enaliosaurs, which in this respect agree with many Lacertians.

The special characteristic of the dorsal vertebræ in the *Pl. macrocephalus*, as compared with the *Pl. Hawkinsii*, consists in their being, like the cervical, more flattened in the antero-posterior direction and more concave at the sides; in which latter particular they resemble the dorsal vertebræ in the *Pl. dolichodeirus* more than those of the *Pl. Hawkinsii*.

At the commencement of the dorsal series the lower margin of the neurapophyses is angular, as in the neck; but towards the middle of the back they become rounded, and the articular depressions in the body of the vertebra present a corresponding form. The transverse processes progressively increase in length towards the middle of the trunk, and again diminish as they approach the tail; the bases of the neurapophyses from which they rise diminish in vertical extent in the same ratio, and leave a greater proportion of the centrum free from their embrace. The increasing length and upward inclination of the transverse processes supporting the ribs, Mr. Conybeare has justly observed, "seem intended to give a wider sweep to the ribs," and relate to the acquisition of greater expansion of the thoracic-abdominal cavity at the part where the largest viscera were lodged. The spinous processes at the beginning of the dorsal region diminish in antero-posterior extent, but slightly increase in height; they then increase in both dimensions to the middle of the back, and thence gradually decrease to the tail.

Sacral Vertebræ.—There are no sacral vertebræ by anchylosis in the Plesiosaurs. In Lord Cole's remarkable specimen of

the present species the costal articular surface begins to descend from the neurapophysis upon the body at the fiftieth vertebra, and this and the succeeding vertebra may consequently be reckoned as sacral; their relative position to the dislocated ileum verifies the supposition that the same character, with reference to the costal articular surface, points out the sacral vertebræ in the *Pl. macrocephalus*, as it does those of the *Pl. Hawkinsii*. The surface which supported the spinal marrow in the sacral vertebra is small and flattened, slightly impressed, bounded by two gently curved lines, whose convexities are turned towards each other. A comparison of the medullary canal at this part and at the cervical region shows that the usual law of the increase of the spinal cord at the parts where larger nerves were required to be given off to supply the locomotive extremities, obtained in the extinct Enaliosaurs, but only in the same degree as in their existing cold-blooded congeners.

In the caudal vertebræ the length of the centrum is to its transverse diameter as 2 to 5.

Extremities.—In the bones of the paddle, or radiated appendage of the pectoral arch, the following differences exist between the *Pl. macrocephalus* and *Pl. Hawkinsii*. The humerus, as was before observed in the description of the *Pl. Hawkinsii*, is less contracted at its proximal extremity, and less curved backwards; the anterior margin being rather concave, instead of convex as in the *Pl. Hawkinsii*; it is also broader in proportion to its length; its greatest breadth (at the distal extremity) being more than half its length, while in *Pl. Hawkinsii* it is less, being about $\frac{3}{8}$ths of the same length. The distal end terminates in a slight but regular convex curve, while in *Pl. Hawkinsii* the separate facets for the radius and ulna are distinctly marked, and meet so as almost to form an obtuse angle.

In the Plesiosaurs generally the *radius* is nearly straight, while the ulna is bent with the concavity towards the radius: both bones are flattened, as in other Enaliosaurs. In *Pl. macrocephalus* the margin of the radius next the ulna is more concave than in *Pl. Hawkinsii*, and the bone is relatively broader at its distal extremity, which is terminated by a convex, instead of a nearly straight, line. The ulna equals in length the radius, as in the *Pl. dolichodeirus*, while it never attains the same length in the *Pl. Hawkinsii*; it is also relatively broader than in either the *Pl. dolichodeirus* or *Pl. Hawkinsii*; and presents a more regular reniform figure; the humeral articular surface not being so straight, or so distinctly marked off from the outer convex margin. The carpus consists, in the *Pl. macrocephalus*,

of eight instead of six ossicles, as in the *Pl. Hawkinsii.* The fourth or additional one in the first or proximal row is wedged in between the ulna and the third carpal bone, at the outer angle of the carpal joint; it is much smaller than the rest.

The relative sizes also of the three normal bones of the first row is different; in the *Pl. Hawkinsii* the middle one is the largest, the radial or anterior one least: in the *Pl. macrocephalus* the ulnar or posterior of the three is, if anything, the largest, and the radial bone not so much smaller than the other two. The disproportionate size of the two posterior bones in the *Pl. Hawkinsii* compensates for the shortness of the ulna. In the distal row of the carpus the superadded bone in *Pl. macrocephalus* is a very small ossicle wedged in between the third or posterior carpal, and the fifth or ulnar metacarpal bones.

The metacarpal bones correspond with those of *Pl. Hawkinsii*; the radial or anterior one, which corresponds to the pollex, being the shortest and broadest.

	*Pl. macrocephalus**.	*Pl. Hawkinsii.*
The 1st or radial metacarpal supports	2 (but probably 3 phalanges)	3.
The 2nd metacarpal	6	6 or 7?
The 3rd ditto	9	8 or 9?
The 4th ditto	8	8.
The 5th ditto	6	6*.

The evidently natural curve formed by the distal phalanges in Lord Cole's *Pl. macrocephalus* indicates that the paddles were more flexible at their tapering extremity than those of the modern *Cetacea.*

Pelvic Extremity.—The femur in *Pl. macrocephalus* is relatively longer than in *Pl. Hawkinsii* or *dolichodeirus.* In the latter it equals the humerus in length; in the former it exceeds the same bone by one eighth.

In the *Pl. macrocephalus* it is rather more expanded at the distal extremity than in the *Pl. Hawkinsii*, but the difference of form is not so well marked as in the humeri of these two species. The bones of the leg have the same distinguishing character as those of the fore-arm; and the fibula, in all the Plesiosaurs, corresponds to the ulna in its peculiar bent figure.

In the *Pl. macrocephalus* the fibula is, however, relatively broader than in the *Pl. Hawkinsii*, notwithstanding that, like the ulna in the fore-arm, its distal extremity is on the same plane with that of the adjoining bone. It is, in fact, fully as broad as

* In the enumeration of phalangeal bones by Mr. Conybeare (*Geol. Trans.* 1824, p. 387,) the metacarpals are included; allowing for this, the perfect digits of the *Pl. dolichodeirus* correspond in number with *Pl. Hawkinsii* and *macrocephalus.*

it is long; which proportions distinguish it from the fibula of either the *Pl. Hawkinsii* or *Pl. dolichodeirus*.

The tarsus consists, in *Pl. macrocephalus*, of six, instead of five bones as in the *Pl. Hawkinsii*. It participates in the peculiarity of having those bones, which are situated at the anterior or tibial side of the joint, much smaller than those of the fibular side, and so placed between the tibia and tibial metatarsals as to indicate that the foot had a freer inflection forwards, or upon the tibia, than in the opposite direction.

In the *Pl. Hawkinsii* the interspace between the tibia and metatarsals is occupied by a single round flat bone; but in the *Pl. macrocephalus* by two; the additional bone being situated at this part of the tarsus.

The metatarsals resemble in number and disposition those of the *Pl. Hawkinsii*. In the general form and proportions of the phalanges of both extremities a close resemblance exists between the two species.

Localities.—This species occurs in the lias of the valley of Lyme: also, but more rarely, in the lias of Street. I have seen detached vertebræ of the *Pl. macrocephalus* from the lias of Weston near Bath.

The vertebræ of the *Plesiosaurus*, included by Professor Jaeger in his list of the fossils of the lias of Boll in Wirtemberg, approach more nearly to the characters of the *Pl. macrocephalus* than they do to any other well-determined species.

Plesiosaurus brachycephalus.

This species, in the strength and comparative shortness of its neck, and in the proportions of its extremities, is most nearly allied to the *Plesiosaurus macrocephalus*; but it differs from that species in the form of its head, and in the character of its cervical vertebræ.

In the nearly complete skeleton of the *Pl. brachycephalus*, preserved in the Museum of the Philosophical Society of Bristol, 75 vertebræ may be counted, and only a few seem to be wanting from the extremity of the tail. Of these at least twenty-eight may be reckoned as cervical, according to the characters assigned to this series of vertebræ in the introductory part of the present Report. The length of the bodies of these vertebræ does not quite equal their transverse diameter. The vertical diameter of the body of the 13th cervical vertebra was 1 in. 5 lines, the antero-posterior diameter 1 in. 2 lines. The anterior and posterior articular surfaces of the body are gently but regularly concave without any median convexity. The costal depres-

sions are elliptical, narrower than in the *Pl. macrocephalus*, but broader than in the *Pl. Hawkinsii*. In the 20th cervical vertebra the costal is situated immediately below the neurapophysial pit; but in the vertebræ anterior to this, they are separated from the neurapophysial pits by a space equal to their own breadth. This is a character which distinguishes the present species very satisfactorily from the *Pl. macrocephalus*. From the *Pl. Hawkinsii* it differs in the greater relative size of the cervical vertebræ; and especially in the superior height of the neurapophyses, which fully equals the vertical diameter of the vertebral body.

The anterior and posterior margins of the sides of the vertebral body are impressed with fine irregular longitudinal ridges, but the midspace between the costal and neurapophysial depressions is quite smooth. The spines of the anterior cervical vertebræ are obliquely truncate at the anterior margin, and are less square-shaped and less strong than in the *Pl. macrocephalus*. The characteristic vascular foramina on the inferior surface of the centrum lie in deep concavities, and are separated by a longitudinal ridge. This ridge gradually disappears in the dorsal vertebræ, and the vascular foramina become more widely separated and approach the lateral aspects of the centrum. The length of the 15th cervical vertebra is 1 in. 8 lines, its vertical diameter 2 in. 2 lines.

From the position in which the vertebræ lie in their lias matrix, it would seem that an elastic intervertebral cushion of from two to three lines thick had been interposed between their bodies. The cervical ribs have their expanded or hatchet-shaped extremities supported on longer pedicles than in the *Pl. dolichodeirus* or *Hawkinsii*, and in this respect they resemble those of the *Pl. macrocephalus*. The spinous processes of the dorsal region are stout and broad, quadrilateral, truncate above, and with the angles very slightly rounded off; they are somewhat longer than those of the cervical vertebræ. The caudal vertebræ appear to be less numerous than in the *Pl. dolichodeirus* or *Pl. Hawkinsii*.

The caudal ribs appear to be expanded at their extremities, somewhat analogously to the cervical ribs; the depressions on the sides of the vertebræ to which they are articulated are round, and have their margins raised; they are situated immediately beneath the neurapophysial pits in the posterior caudal vertebræ. The under part of the caudal vertebræ is concave, and presents two vascular foramina on each side, separated by wide interspaces.

The head is shorter in proportion to its breadth than in any of the previously described *Plesiosauri*; whence the specific name proposed for this species.

There are twenty-six teeth on each side of the lower jaw, the terminal portion of which is less abruptly expanded than in the *Pl. grandis* or *arcuatus*. One of the large anterior teeth of this species measures one inch and a half in length and one third of an inch in breadth; its transverse section is nearly circular. The crown of the teeth is sculptured with well-marked and finely-waved longitudinal grooves.

The breadth of the distal end of the humerus equals half the length of the bone, which is nine inches; the form of that bone is the same as in the *Pl. macrocephalus*. The other bones of the anterior paddle are not sufficiently complete to aid in characterizing the present species. In the posterior extremity the ischium differs from both that of *Pl. dolichodeirus* and *Pl. Hawkinsii* in the greater width and less deep concavity of its anterior margin; from which may be inferred a corresponding modification of the pubis, where that bone combines with the ischium to complete the abturator foramen. The mesial margin is more convex than in the *Pl. Hawkinsii*.

The femur is a tenth part longer than the humerus, and its distal extremity is relatively less expanded: its anterior margin is less concave than in the *Pl. macrocephalus*. It measures nine inches nine lines in length.

The tibia is somewhat narrower and its anterior and posterior margins less curved than in the *Pl. macrocephalus*. The fibula approaches more nearly in form to that of the *Pl. macrocephalus* than to the fibula of any other species of *Plesiosaurus*; but its tibial margin is more extended and less deeply concave. Its breadth is equal to its length, which is only two lines less than that of the tibia. The tarsal bone next the interspace of the tibia and fibula is not emarginate.

The total length of the skeleton of the *Pl. brachycephalus* from the Bitton lias is ten feet and a half. The length of the head is one eighth of the entire length of the skeleton, or equals the nine anterior cervical vertebræ.

Localities.—The incomplete skeleton of this species in the Museum of the Bristol Philosophical Institution was discovered in the lias at Bitton, Gloucestershire, in 1830.

Several vertebræ in the Gymnasium at Stuttgard, from the lias of Boll, appear to belong to the present species.

Vertebræ of the *Pl. brachycephalus* also occur in the lias at Whitby.

I have not met with any specimens from the Dorsetshire or Street lias referrible to this species.

Plesiosaurus macromus.

In the *Pl. dolichodeirus* and *Hawkinsii*, which may be regarded as the typical species of the present most singular genus, the anterior and posterior paddles are of equal size. The *Pl. macrocephalus* and *brachycephalus* are distinguished in addition to other characters by the superior length of the hinder paddles. In the present species the contrary proportions prevail; here we find at length an instance in which the *Plesiosaurus* resembles the *Ichthyosaurus*, in the superior size of the anterior as compared with the posterior extremities; and the equality of the locomotive members, as respects their length, proves to be a specific and not a generic character. A considerable proportion of the skeleton of the *Pl. macromus* was discovered by Miss Anning in the lias near Lyme, and now forms part of the valuable collection of Miss Philpotts. These interesting remains include the greater part of the vertebral column, with the principal bones of the anterior and posterior extremities; but the skull and teeth are unfortunately wanting.

The cervical vertebræ resemble those of the *Pl. dolichodeirus* in their general form and proportions, and in the relative position of the neurapophysial and costal surfaces in the anterior cervical vertebræ, but differ in the character of the articular surfaces of the centrum. The body of one of the vertebræ from the middle of the neck gives the following admeasurements:

	Inch.	Lines.
Length or antero-posterior diameter. .	1	4
Vertical diameter	1	4
Transverse diameter	1	6

The anterior and posterior articular surfaces are gently and uniformly concave, without the central rising described by Mr. Conybeare in the *Pl. dolichodeirus*, and which is present in some other species. In many of the vertebræ of the present specimen there is a transverse linear impression in the centre of the above-mentioned articular surfaces. The lateral surfaces are sculptured at the anterior and posterior margins by numerous longitudinal irregular grooves; the intermediate surface is comparatively smoother. A narrow vertical ridge is continued from the descending angle of the neurapophysial depression to the middle of the upper costal surface. The distance which intervenes between the neurapophysial and costal

articular surfaces is nearly equal to the vertical diameter of the latter. The costal pits are slightly raised above the surrounding surface; they are of a transversely elliptic form, and with a greater vertical breadth than usual; they are also separated by a deeper and wider channel than in the *Pl. Hawkinsii.* The under surface of the vertebræ is traversed along its middle by a smooth broadish ridge, on each side of which is the characteristic foramen, of an elliptical form. The longitudinal ridge disappears in the dorsal vertebræ. The sides of the neurapophysial depressions meet at nearly a right angle. The spinous process is less extended in the antero-posterior direction than in the *Pl. macrocephalus,* but is thicker (transversely) than in the *Pl. dolichodeirus.*

The dorsal vertebræ are a little shorter than the cervical: they have the same anterior and posterior articular surfaces; but the lateral surfaces are smoother and more concave. In the caudal vertebræ the inferior surface of the centrum is flattened: the hæmapophysial pits are well marked, especially the anterior part. The humerus of this species has the same form as the bone of the *Plesiosaurus* figured by Mr. Conybeare in the 1st Vol. 2nd Series of the *Geol. Trans.* pl. xxii. fig. 1: it is expanded immediately below the head by the development of two rough protuberances for the insertion of muscles: below these the shaft of the bone continues to diminish gradually in thickness and increase in breadth to its distal extremity. The contour of the anterior and posterior margins resembles that of the same parts in the *Pl. dolichodeirus,* the anterior being slightly convex, the posterior in a greater degree concave: the distal extremity is bounded by a pretty regular convex curvature.

The rough muscular surfaces on the inner side of the bone near the proximal extremity are only slightly raised above the smooth surrounding surface, and they are so close together as to be nearly confluent. The external tubercle is as it were separated by a compression from the general proximal surface. At the distal extremity the rough articular surface is divided into two parts by a shallow depression on the internal or sternal side of the bone; at the junction of the middle with the posterior third of the distal curve,

	Inches.	Lines.
The length of the humerus is	8	6
The breadth of the distal end . . .	3	10

The radius presents the common form, is more expanded at the proximal than the distal end, compressed, and with the anterior and posterior margins concave. The ulna is as long as

the radius, and resembles both in this proportion and in its shape that of the *Pl. dolichodeirus*: its radial or anterior concavity is less deep, and the posterior margin less convex than in the *Pl. Hawkinsii* or *Pl. macrocephalus.*

	Inches.	Lines.
The length of the ulna is	3	6
The breadth	2	2

The femur differs in form as well as in size from the humerus: its anterior as well as posterior margin is slightly concave; and it is upon the whole somewhat shorter and thicker in proportion to its length. Near the proximal extremity there is a rough muscular protuberance corresponding with that in the humerus, but more circumscribed and more raised. The distal end is bounded by a pretty regular convex curve, and its rough articular surface is in the form of a compressed ellipse, not encroached upon by any depression as in the humerus.

	Inches.	Lines.
Length of the femur	7	8
Breadth of its distal end	3	6

The tibia like the femur is thicker as well as shorter than the corresponding bone in the fore-arm.

	Inches.	Lines.
Length	3	0
Breadth of the proximal end	2	3
——— distal end	1	7

The remaining bones of the extremity are not sufficient in number to enable me to recompose the entire extremity so as to compare together the bones of the hand and foot: but the proportions above described of the larger bones of the anterior extremities are sufficient to distinguish the present from any other known species of Plesiosaur. The only doubt which occurred was with reference to the fact of the humerus and femur having actually belonged to the same skeleton; but the correspondence of the bones in texture, colour and general appearance, and in the rough surfaces for muscular attachment, coinciding with the evidence of the indefatigable and acute discoverer, Miss Anning, as to the proximity of all the remains of this specimen in the lias matrix, assured me that the comparison might be safely made.

Plesiosaurus pachyomus.

A new species of *Plesiosaurus* is indicated by certain remains, in the collection of Prof. Sedgwick, from the greensand of Reach about six miles from Cambridge.

The humerus, or femur, is remarkable for its thickness, whence the specific name proposed for this species; its distal flattened extremity is one inch and a half thick, the breadth of the same part being only four inches and a half, and the length of the entire bone nine inches and a half. The contour of the head is transversely oval. The central part of the bone is occupied by a coarse cellular structure, one inch and a half in diameter, surrounded by dense osseous walls three lines thick.

The body of a cervical vertebra of this *Plesiosaurus* measures

	Inches.	Lines.
in longitudinal diameter	1	7
in transverse ———	2	3
in vertical ———	1	9
The distance between the neurapophysial and costal pits is	1	0 :

the form of the costal pit is a full transverse ellipse, and there is a transverse depression about three lines in length in the centre of the articular surface of the body. The neurapophysial pits are much wider in the dorsal than in the cervical vertebræ. The sides of the vertebræ are smooth and slightly concave.

Plesiosaurus arcuatus.

Under this name I designate a species of *Plesiosaurus*, of which I have been able to study parts of the skeleton in the British Museum (from the collection of Mr. Hawkins), in the Bristol Museum, and in the collections of Lord Cole and Prof. Sedgwick.

The vertebræ, especially those at the posterior moiety of the cervical region, are characterized by the development of distinct transverse processes from the sides of the centrum for the support of the cervical ribs. These processes have the articular surfaces traversed by a longitudinal groove, as in other *Plesiosauri*, and consequently thus present the appearance of the two normal transverse processes confluent at the base. The articular surfaces on the anterior and posterior sides of the body of the vertebra present the true Plesiosaurian character, being slightly concave with a gentle central convexity. The lateral and inferior surfaces of the vertebral centre are smooth and concave. The neurapophysis from its base to the summit of its oblique process is equal in vertical extent to the rest of the vertebra below: the inner surface of the neurapophysis is traversed in its middle by a longitudinal ridge, (at least in the posterior cervicals,) probably for the attachment of the membrane of the spinal cord. The spinous process of the neurapophyses is high

and broad, but is more particularly characterized by the lateral expansion of its summit, the anterior angle of which is obliquely truncated, presenting at that part a flattened surface.

The vertical extent of the entire cervical vertebra here described is 7 inches. The vertical diameter of the centrum $2\frac{3}{4}$ inches; its transverse diameter the same; its antero-posterior diameter $2\frac{1}{3}$ inches. In its general proportions this vertebra has most resemblance to the corresponding one of the *Pl. macrocephalus*, but it differs in the presence of the transverse processes, and in the form, and especially the superior expansion of the spine.

The dorsal vertebræ are distinguishable by the correspondingly great development of the transverse processes upon the neurapophyses.

The episternal bone of this species is figured in Mr. Hawkins's 26th Plate. It measures 16 inches and $\frac{1}{3}$rd in transverse extent. The arc of its anterior concavity, which is less deep than in *Pl. Hawkinsii*, measures 8 inches. The lateral alæ are slightly convex externally, and terminate posteriorly at an acute point. It is much flattened, thickest in the middle, from the internal surface of which a longitudinal ridge is produced.

The humerus of the same skeleton measures 14 inches long, and $7\frac{1}{2}$ inches broad at its distal extremity. The anterior surface is slightly concave, as in the *Pl. macrocephalus*. The tuberosity of its head is more raised than usual in the *Plesiosauri*.

The dentary piece of the lower jaw contains the alveoli of 60 teeth; the diameter of the base of the large anterior ones in the expanded symphyseal portion equals $\frac{2}{3}$rds of an inch. The length of this dentary piece is 13 inches. The diameter of the expanded symphysis 4 inches.

In the collection of Miss Philpotts at Lyme Regis there is a dentary bone which appears to be referrible to this species, but it is smaller, measuring 13 inches in length and 3 inches across the expanded symphysis; it contains the alveoli of $27—27=54$ teeth, of which the six anterior ones on each side are larger than the rest, and occupy the first four inches of the bone on each side. The projecting crown of one of these teeth measured 1 inch 3 lines, and the breadth of its base $5\frac{1}{2}$ lines.

A femur, supposed to belong to the same *Plesiosaurus*, measured 12 inches in length, and 6 in breadth at the distal end: its anterior margin was slightly concave; the shaft smooth, and the extremities longitudinally striated: near the proximal end ($2\frac{1}{2}$ inches below it) there is an oval muscular surface, measuring 2 inches by 1 inch.

Localities.—The remains of this species which I have exa-

mined are from the lias of Street and the neighbourhood of Bath; also from the lias of Bitton, in Gloucestershire; and from that of Charlton, two miles from Cheltenham. I have not yet observed specimens from the lias of Whitby.

None of the Plesiosaurian vertebræ of the Wirtemberg lias bone-beds can be referred to the present species.

Plesiosaurus subtrigonus.

I have next to notice some vertebræ of a *Plesiosaurus* contained in the Museum of the Philosophical Institution at Bristol, which approximate to the character of the last-described species in the prominence of the costal articular surfaces, but which differ in the shape of the body in so marked a degree as to render inadmissible the idea of their specific identity. In the *Pl. arcuatus* the contour of the articular surface is nearly circular, with a narrow superior emargination corresponding with the canal for the spinal marrow: in the present species the contour of the same part approaches to the triangular form from the flatness and upward convergence of the upper moiety of the sides of the body.

The anterior and posterior articular surfaces are flatter, but exhibit a slight middle convexity.

The inferior surface is traversed by a broad longitudinal elevation, on each side of which is the typical foramen. From the median ridge the surface of the vertebra extends in a slight concave curve to the base of the transverse process, the antero-posterior extent of which equals one half of that of the centrum itself.

The base of the neurapophyses is bounded by a gentle convex line, and the extent of surface intervening between it and the costal articular surface exceeds by one half the vertical diameter of that surface.

The upper part of the neurapophyses, and their spine, were broken off in the vertebræ examined by me, the only ones of this species which I have as yet seen.

	Inches.
The length of the body of the cervical vertebræ here described is	$3\frac{1}{3}$
Its transverse diameter, including transverse processes	$4\frac{1}{2}$
Its vertical diameter	$3\frac{1}{2}$

Localities.—The vertebræ here described are from the lias of Weston, near Bath*.

* In Professor Sedgwick's collection at Cambridge there is a cervical vertebra of a *Plesiosaurus*, measuring two inches and a half in transverse diameter, having the neurapophyses anchylosed to the centrum, and the ribs to the

Plesiosaurus trigonus, Cuv. (?)

This species was founded by Cuvier on a vertebra from the coast of Calvador, which presented a triangular form, like some of those of the *Mosasaurus*, *i. e.*, it was flat and broad below, and gradually decreasing above; on the sides of the lower surface were transverse processes. As the form of the articular surfaces are not described, it may be questioned whether the vertebra above alluded to really belongs to the Plesiosaurian type: and in the present instance these surfaces deviate somewhat from the characteristic form.

They are flat on the outer or peripheral third, and the remaining central part, instead of being convex, as in the *Plesiosauri* generally, are slightly concave. It is this character, in addition to their smaller size, which principally distinguishes the present from the last-described vertebra, with which it otherwise closely agrees in its leading characters, viz. those afforded by the development and position of the transverse processes. These transverse processes are short and thick; the breadth or antero-posterior diameter of their base equals one half of that of the entire centrum; they are directed obliquely downwards.

The contour of the articular surface of the body of the vertebræ is nearly circular: the triangular figure is due to the position and direction of the transverse processes; the distance between these and the neurapophyses equals twice their vertical diameter: and although this character will be lost in the more posterior cervical vertebræ, it is to be remembered that it is one which has never presented itself in those *Plesiosauri* of which the entire series of cervical vertebræ have been examined.

	Inches.
The antero-posterior extent of the vertebra described is	$1\frac{2}{5}$
Transverse diameter, including the transverse processes	$2\frac{1}{2}$
Vertical diameter of the body	2

This vertebra is from the lias of the bone-bed of the Aust Cliff, near Bristol.

Plesiosaurus brachyspondylus, O.
——— *recentior* (?), Conybeare*.
——— *giganteus* (?) *Ibid.*†

transverse processes, which are as remote from the neurapophyses as in the *Pl. subtrigonus*; but the form of the articular surface of the vertebral centre is more regularly rounded. This vertebra, which seems to indicate a new species or subgenus of Enaliosaurs, is from the lias at Bridport, Dorsetshire. It is recommended that the Saurian fossils of this locality be carefully searched for and preserved.

* Herm. Von Meyer, *Palæologica*, p. 112. † *Geol. Trans.*, 1824, p. 389.

In the museum of Viscount Cole, at Florence Court, there are some detached vertebral centres, which, from their remarkable compression in the antero-posterior direction, resemble those of the *Ichthyosaurus*, but which combine the peculiar Plesiosaurian structure with this character.

The articular surfaces for the contiguous vertebræ are very slightly concave, with a small round depression, but no convex rising at the centre. The sides and under part of the body are concave: the surface is tolerably smooth, and the two usual vascular perforations are present at the lower part of the body. I have figured one of these vertebræ from the posterior part of the cervical series, where the costal articular surface is continuous with the neurapophysial one, but has not wholly risen above the centrum.

The costal surface stands out a short way from the level of the lateral surface of the vertebra in the form of a compressed vertically elongated transverse process.

The neurapophysial depression is shallow, occupies the whole breadth of the vertebra, and presents a convex edge next the spinal canal.

The part of this canal due to the centrum is in the form of a concave depression widened at both ends, but more so posteriorly.

Localities.—The vertebræ here described were from the Kimmeridge clay, Heddington Pits, near Oxford.

There are similar vertebræ in the collection of the Philosophical Society of Bristol, from the Kimmeridge clay near Weymouth. These appear to be the vertebræ figured by Mr. Conybeare in Pl. XXII. vol. i. Second Series of the *Geol. Trans.*, also from the Kimmeridge clay near Weymouth, belonging to the same species. They present the same compressed form and central pit on the anterior and posterior articular surfaces, and prominent costal articular surfaces. These figures are referred to by Cuvier and V. Meyer as the type of the *Plesiosaurus recentior* of Conybeare. They are alluded to by Mr. Conybeare in a subsequent memoir as belonging to the same species as the gigantic fragments obtained by Professor Buckland at Market Raisen, and which are provisionally indicated under the name of *giganteus*. The most striking peculiarity of this species is, that the anterior cervical vertebræ are even more compressed in the antero-posterior direction than the posterior cervical vertebra above described, while the vertebræ in the dorsal region regain more of the ordinary Plesiosaurian proportions, although still narrower in the antero-posterior direction than in any of the previously described species; hence we may conclude that the neck was shorter in the *Pl. brachyspondylus* than in the other

Plesiosauri, and it may be inferred that it had a large and heavy head.

As there are other species of *Plesiosaurus* from strata as recent as those which contain the remains of the present, and as there are at least two species of *Plesiosauri* of gigantic proportions, one of which has the humerus and femur of the ordinary conformation, while the other has the same bones distinguished by large trochanterian processes, the aim which I have in view to indicate as precisely as possible the different species of *Plesiosaurus* that have characterized our strata, seemed to be best answered by giving a name to the present species indicative of a structure which appears to be peculiar to it.

Plesiosaurus costatus.

Under this name I provisionally indicate a species characterized by a well-marked vertebra from the bone-bed of the lias at Aust Cliff, near Bristol.

In its general form and proportions, this vertebra (which is is one of the anterior or middle cervicals) comes nearest to the corresponding vertebra of the *Plesiosaurus macrocephalus*, and presents a still more important character of agreement in the contiguity of the costal and neurapophysial articular depressions. I have already observed that the costal surfaces rise a little above the level of the vertebra in the *Plesiosaurus macrocephalus:* in the present specimens they are supported on two distinctly developed articular processes, the intervening groove of which is deeper and broader than in the *Plesiosauri* generally. The size of the two costal surfaces combined indicates the hatchet-shaped rib to have been relatively larger than usual. The free surface of the vertebra is more irregular than in the *Pl. macrocephalus*; it is marked in a characteristic manner by irregular ridges near the raised circumference of the anterior and posterior articular surfaces, from which rugged boundaries deep and pretty broad grooves pass in a nearly parallel direction towards the middle of the vertebra. The lower surface is traversed by a strongly developed median longitudinal ridge, on each side of which is the large characteristic vascular foramen. The articular surface on the anterior and posterior sides of the body deviates from the typical Plesiosaurian character in being more concave in the centre than at the circumference, instead of the reverse condition. About one fifth part of the articular surface next the periphery is flat. The remaining median concavity is slightly marked.

The articular base of the neurapophysis is triangular, and the two sides converge downwards at a more acute angle than I have

observed in the corresponding part of any other Plesiosaur. The distance from the upper margin of the posterior oblique process to the inferior apex of the neurapophysis exceeds the vertical diameter of the body, and is double the extent of that part of the centrum below the neurapophysis. The articular base of the neurapophysis extends inwards above the centrum, so as to form part of the floor of the spinal canal: the inner surface of the neurapophysis is traversed by a longitudinal ridge.

The spine of the vertebræ here described was unfortunately broken off.

I have not met with any other example of a Plesiosaurian vertebra agreeing with the one above described, except a few from the Bristol lias above specified.

Among these is a centrum of the dorsal series of vertebræ, which, from the character of the anterior and posterior articular surfaces, and the rugose lateral and inferior surfaces, belongs, in all probability, to the same species. It is relatively more compressed in its general form: but we may observe the same difference between the cervical and dorsal vertebræ of the ***Pl.*** *dolichodeirus*.

Admeasurements of the above-described Cervical Vertebra.

	Inches.	Lines.
Antero-posterior diameter of the body . .	1	6
Transverse diameter of the body	2	0
Vertical diameter	1	9

Dorsal Vertebra.

Antero-posterior diameter of body	1	6
Transverse diameter	2	9
Vertical diameter	2	6

*Plesiosaurus dœdicomus**.

Of this well-marked species the *humerus* is preserved in the collection of Sir Philip Egerton: the specific name relates to the peculiar form of that bone, which resembles a flattened spoon in the narrowness of the shank or shaft, and the remarkable expanse of the distal extremity. The breadth of this part equals one half of the entire length of the bone, and is five times greater than the breadth of the proximal extremity. The shaft gradually expands from the proximal end, and exhibits the flattened form characteristic of the genus. There is no trace of a tuberosity at the proximal extremity.

* δοιδὺξ, a spoon; ὦμος, *humerus*, or arm-bone.

From the analogy of other *Plesiosauri* I should conceive the femur to have corresponded with the humerus in the peculiar form above described; but as yet I have not met with an example of this bone.

Localities.—This bone was from the Kimmeridge clay at Shotover, near Oxford.

In the collection of Professor Sedgwick, at Cambridge, there is an imperfect gigantic paddle, of which the first bone (whether humerus or femur is not determinable in its detached condition,) presents an expansion of the distal extremity hardly less disproportionate than that above described: the length of this bone is sixteen inches, the breadth of the distal extremity is eleven inches. There are ten of the smaller bones of the paddle associated with the above, and presenting the Plesiosaurian type, but without modifications worthy of being specified. The specimen was discovered by Captain Smith in the Oxford clay near Bedford.

Plesiosaurus rugosus.

In three museums, viz. that of Bristol, of York, and of Viscount Cole, I have observed Plesiosaurian vertebræ which are readily distinguishable from all other vertebræ by the peculiarly rugous character of the free or non-articular surfaces of the body. But this superficial modification is not the only character by which these vertebræ may be distinguished.

The most characteristic vertebræ, viz. those from the middle of the cervical region, although they present modifications of the neurapophyses and costal articular surfaces resembling those characteristics of the *Pl. Hawkinsii* more nearly than any other species, yet differ therefrom in the following particulars. The two costal impressions on each side are completely divided, and by a wider and a deeper groove: they are situated nearer the lower margin of the vertebra; and an extent of surface equal to twice the vertical diameter of the combined surfaces intervenes between them and the base of the neurapophysis. This is bounded by a more open angle than in the *Pl. Hawkinsii.* The distance from the lower margin of the neurapophysis to the articular surface of the posterior oblique process, is only a little more than half the extent of the centrum below the neurapophysis, a proportion which I have not yet met with in any other species, but to which the vertebræ in the *Pl. Hawkinsii* offer the nearest approach.

The contour of the articular surface of the vertebral body is almost circular: the peripheral border of this surface is convex, which leads inwards to a concavity, and the centre of the surface again rises in a slightly convex form.

The bodies of the vertebræ are relatively longer than in the *Pl. Hawkinsii*, being intermediate in this respect between it and the *Pl. dolichodeirus*.

Localities.—The vertebræ of this species occur in the lias at *Lyme Regis*, in that of the Aust Cliff, Bristol, and in the neighbourhood of Whitby.

Plesiosaurus grandis.

From the greensand, Kimmeridge clay, and Oxford clay, many specimens have been obtained of humeri or femora, closely corresponding with the Plesiosaurian type of these bones, but of gigantic size, and belonging to two distinct species of Enaliosaurian reptiles.

The long bone (*humerus* or *femur*) indicative of the first of these species, presents the ordinary rough, elliptical, slightly convex head, beneath which the bone slightly contracts to about one third of the distance from the opposite end; it then begins gradually to expand in breadth, and to decrease in thickness to the distal extremity, which is terminated by a pretty regular semicircular contour. The anterior margin is slightly concave; the posterior one more concave. Beneath the head or proximal end of the bone there is a rough surface for muscular attachment, but no process or trochanterian prominence. The length of one of these bones in the collection of Viscount Cole is sixteen and a half inches. The breadth of the narrowest part of the bone is three inches and two thirds, that of the broadest part eight inches. The surface of the upper third of the bone is roughened with small perforations and longitudinal ridges; the surface of the lower two thirds is smooth, except near the articular expanded extremity, which is pitted with small vascular grooves.

A second long bone in the same collection, having similar characters to the preceding, measures fourteen inches in length, seven inches in breadth across the distal extremity, and nine inches in girth round the middle of the bone: this is probably the humerus.

A third long bone, also in the collection of Viscount Cole and from the Kimmeridge clay at Shotover, belonging to the same species as the preceding bones, measures seventeen inches and a half in length, seven inches and one third across the distal end, and nine inches in girth. The contour of the distal articular end is rounded as in the first-described bone, and it is probable that they are both femora; but as the proportions of the humerus and femur are not constant in the genus *Plesiosaurus*, the nature of the above-described bones cannot be

certainly determined until some perfect specimens are met with.

From the same member of the upper oolite system, and from the same locality, Shotover, Lord Cole possesses a well-preserved large triradiate flattened bone, which, from its correspondence in colour and grain with the humeri and femora above described, seems unquestionably to belong to the same species of *Plesiosaurus*, of which it represents the scapulo-clavicular bone.

The longest diameter of this bone, viz. from the end of the ray representing the scapula to the sternal end of the clavicle, is nine inches. The breadth of the scapular ray is three and a half inches; both this ray and the one representing the sternal end of the clavicle are much flattened, not exceeding half an inch in thickness: the third ray, which represents the humeral end of the scapula and clavicle, is short and thick, and terminates in a rough convex articular surface, part of which joins the coracoid, and the rest contributes to form the glenoid cavity for the humerus. The external and internal surfaces of this bone are pretty smooth, but exhibit the lines or striæ of growth, which radiate from the centre of the bone.

Near the place where the above-described bones were deposited, there was also found an ischium so closely corresponding with them in size, colour, surface, and general condition, as to leave little doubt of their being parts of the same species if not individual. The length of this bone, taken between the two extremities of its median margin, is twelve inches; the distance from the anterior of these margins to the anterior edge of the acetabular surface four inches. As this bone confirms the indications of the specific difference of the *Plesiosaurus* under consideration, a few words as to its modifications in other species may here be useful. The median margin of the ischium in the *Pl. Hawkinsii*, *e. g.*, is straight, and is joined to that of the opposite bone, except at the angles, which are rounded off.

In the *Pl. dolichodeirus* only the upper half of the corresponding margins of the ischia are sufficiently straight and parallel to be in contact; the lower half of the median margin of each bone receding from its fall in a gentle convex line.

In the *Pl. brachycephalus* the whole contour of the median margin of the ischium is convex, but least prominent at the middle part. In the present large species the corresponding margin is still more convex, so that it could only come in contact with the opposite ischium at one point of this margin.

The anterior concavity, which cooperates with the pubis in

the formation of the foramen ovale, is relatively shorter than in the ischia of any of the preceding species; and the neck of the bone, or that end or angle which enters into the formation of the acetabular cavity, is relatively shorter and broader. The corresponding part of the scapulo-clavicular bone, which corresponds with the ischium, presents the same distinctive character. The acetabular surface is rough, convex, and with a ridge along the middle of its long axis.

Plesiosaurus trochanterius.

The long bones of the second gigantic Plesiosauroid species, from the Kimmeridge clay, deviate from the usual structure of the humerus and femur in that genus in having a strongly-developed trochanterian ridge projecting from the outer side of the head of the bone: this process is of considerable breadth, stands well out from the surface at its upper part, then gradually subsides, and is lost in the upper third of the humerus. The shaft of the bone is more cylindrical than in most of the *Plesiosauri*, and the distal expanded extremity is of less relative breadth. The whole surface of the bone is roughened by longitudinal furrows, ridges and foramina; and on the inner side of the bone, about one fourth of the length of the bone from its head, there is a transversely elongated very rough surface for the implantation of muscle.

One of these long bones in the museum of Viscount Cole measures two feet in length, nineteen inches in circumference at the head, including the trochanter, twelve inches and a half round the middle of the shaft, and ten inches across the flattened distal end; this is terminated by two slightly concave equal articular surfaces, which meet at a ridge or salient angle. The thickness of these surfaces is one inch nine lines; they are each traversed longitudinally by a convex ridge, the base of which is rather more than two thirds the breadth of the articular surface, which is slightly concave on each side of the median ridge.

Locality.—This bone is from the Kimmeridge clay of Shotover Hill. The trochanter here rises as high as the head itself, from which it is separated by a deep and narrow groove.

Professor Sedgwick possesses a humerus or femur of this species, from the brown alluvial clay at Bourn, Cambridge, presenting a single long external trochanter, from which the bone suddenly tapers to the shaft, and then becomes flattened and expanded.

	Foot.	Inches.
Length	1	4
Breadth of distal extremity . . .	0	9
——— middle of shaft . . .	0	4

The proximal surface is pitted like the epiphysial end of a mammiferous femur: it was probably capped by cartilage, and joined by ligamentous substance, without a synovial joint, to the acetabulum.

Plesiosaurus affinis.

In the excellent collection of fossil remains in the museum of Viscount Cole there is a humerus or femur similar to that of the preceding species in regard to the existence of a trochanter, but differing in its smaller development, in the general form of the shaft of the bone, and in size: this bone is only eight inches in length.

The trochanter projects from the outer side of the head of the bone, but its most prominent part is on a level with the inferior margin of the head or proximal articular surface. The breadth of the trochanter is rather more than one third the breadth of the proximal extremity. The trochanter gradually subsides to the level of the shaft, which in the upper fourth of its extent presents with the trochanter a triedral transverse section with the angles rounded off. The shaft begins to be flattened immediately below the trochanter, and gradually to increase in breadth, but it preserves a greater relative thickness than in the larger bone. The general surface is broken by fine striæ and perforations, and there is a well-marked transversely oblong rugosity on the inner side of the upper fourth of the bone.

The differences just specified between this small trochanterian bone and the great one before described show that it cannot have belonged to a younger specimen of the same species. Both bones are solid throughout.

Locality.—Kimmeridge clay, Heddington-pits, Oxford.

CHARACTERS OF THE GENUS *ICHTHYOSAURUS.*

The Enaliosaurians of the present family differ from those of the preceding most remarkably in the shortness of the neck and the equality of the width of the occiput with that of the thorax, which almost immediately succeeds it; impressing the observer with the conviction that the recent animal must have resembled a Cetacean or a Fish in the total absence of any cervical constriction.

This close approximation in the *Ichthyosauri* to the form of the most strictly aquatic animals of the existing creation is ac-

companied by an important modification of the articular surfaces of the vertebral centres, each of which surfaces presents a well-marked concavity, leading to the inference that they were originally connected together by an elastic capsule filled with a fluid, as in the vertebral joints of the back-bone of Fishes, and the Perennibranchiate or most fish-like of the *Reptilia*.

The structure of the fins of many species of *Ichthyosaurus* deviates from that of the Cetacean paddles, and approaches in certain peculiarities more closely to that of the fins of Fishes, than has yet been found in any other reptile. First, the digits exceed the typical number *five*, and resemble in their numerous and small constituent phalanges the jointed rays which support the natatory membrane of the pectoral and ventral fins of true Fishes; and, secondly, numerous cartilaginous bifurcate rays were added to the bony apparatus which supports the tegumentary expansion.

With these important modifications of the head, trunk, and extremities in immediate relation to aquatic progression, the law of the correlations of organic structure would lead us to anticipate some corresponding modification of the tail. Accordingly we find the vertebræ of this part to be much more numerous than in the previously-described Enaliosaurian group. There is no trace, however, of any confluence of the terminal caudal vertebræ, or of any modification of their elongated neur- and hæm-apophysial spines, such as form the characteristic structure supporting the tail of the osseous Fishes. The numerous caudal vertebræ gradually decrease in size to the end of the tail, where they assume a compressed form; and thus the tail, instead of being short and broad as in Fishes, is lengthened out as in the Crocodiles.

The very frequent occurrence of a fracture of the tail about one fourth of the way from its distal extremity, had led me to suspect it to have been connected with the presence of a tegumentary caudal fin; and the laterally compressed form of the terminal vertebræ, since ascertained by Sir Philip Grey Egerton, gives additional demonstration both of the existence and direction of such a fin. The only evidence, in fact, which the skeleton of the Cetaceous mammal affords of the powerful horizontal caudal fin which characterizes the recent animal is the depressed or horizontally flattened form of the terminal vertebræ. We may infer, therefore, from the corresponding vertebræ of the *Ichthyosaurus* being flattened in the vertical direction, or from side to side, that it possessed a caudal tegumentary fin expanded in the vertical direction; and it would be highly advisable to examine narrowly the lias matrix in which the tail of the Ich-

thyosaur may have been imbedded for traces of carbonaceous discoloration, or of an impression of this fin, from which some idea might be formed of its shape and size*.

Thus in the construction of the principal natatory organ of the *Ichthyosaurus* we may trace, as in other parts of its structure, a combination of Mammalian, Saurian, and Ichthyic peculiarities. In its great length and its gradual diminution we perceive the Saurian character; its tegumentary nature, unsupported by osseous rays, bespeaks its affinity to the Cetaceans; while its vertical position brings it close to the peculiar condition of the natatory organ in the Fish.

But, it may be argued, the horizontality of the caudal fin of the *Cetacea* is essentially connected with their exigencies as breathers of the atmospheric air: without this means of displacing a mass of water in the vertical direction, the head of the whale could not have been brought with the required rapidity and facility to the surface to inspire; and as the *Ichthyosaurus* was also an air-breather, a like position of the caudal fin might be considered to be equally essential to its existence in the water.

To this objection it may be replied that the *Ichthyosaurus*, not being warm-blooded, would not need to bring its head to the surface so frequently, or perhaps so rapidly, as the Cetacean; and, moreover, a compensation for the absence of a horizontal terminal fin is provided in the presence of the two posterior extremities, which are modified as paddles, and which are wholly deficient in the *Cetacea*.

Thus I conceive that the living *Ichthyosaurus* must have presented the general external figure of a huge predatory abdominal fish, with a longer tail and smaller caudal fin than usual; scaleless moreover, and covered, according to the minute and careful observations of Dr. Buckland, with a smooth or finely-wrinkled skin analogous to that of the *Cetacea*.

A closer inspection of the enduring parts of these singular inhabitants of the ancient deep, shows that under their fish-like exterior was concealed an organization which, in the main, is a modification of the Saurian type.

Of the Cranium.—The general form of the cranium resembles that of the dolphin, but it differs in the comparatively

* I would more particularly recommend this observation to be made on specimens of *Ichthyosaurus* from the lias of Barrow-on-Soar, which appears to have been more favourable for the preservation of the soft integument than in other localities. The specimen from which Dr. Buckland described the tegument of the abdomen, and that in which the tegument of the fin and the soft rays were described by me, were both from Barrow-on-Soar.

feeble development of the cerebral cavity, and still more essentially in the unanchylosed state of the composite cranial bones, —a fact already referred to in the general characters of the *Enaliosauria*. We shall see, moreover, that the connexions of the bones partake more of the Lacertian than of the Crocodilian types ; but the *Ichthyosaurus* departs at once from both the Cetacean and Saurian characters in the disproportionate development of the intermaxillary, as compared with the maxillary bones, and in the immense size of the orbits and the large and numerous sclerotic plates : it is these modifications which give to the cranium of the *Ichthyosaurus* its peculiar features.

The occipital bone presents in a separate state the usual elementary pieces, called the basi- ex- and supra-occipital bones.

In tracing the analogies of this composite bone, we first observe that in the Crocodile the basi-occipital terminates behind in a convex hemispheric tubercle, which articulates with the wide adontoid appendage of the axis, and with the small body of the atlas. Above the occipital tubercle there is a concave surface on which rests the medulla oblongata. The ex-occipitals articulate with the lateral boundaries of this surface, and form no part of the tubercle for articulation with the atlas.

In the Lacertians the ex-occipitals encroach considerably upon the upper surface of the basi-occipital, diminishing the extent which it affords for the support of the brain, and entering largely into the formation of the articular tubercle by which the head is joined to the vertebral column.

In the *Ichthyosaurus* the ex-occipitals articulate to the whole of the upper surface of the basi-occipital, which sends up a compressed conical crest between them. The ex-occipitals also form a portion of the vertebral articular tubercle, but in a less degree than in most Lacertians. In this respect the *Ichthyosaurus* holds an intermediate position between the Crocodilian and Lacertian Saurians.

The restorations of the posterior region of the Ichthyosaurian cranium hitherto given* are defective in regard to the relative positions of the ex-occipitals to the basi-occipitals ; but the representation by Mr. Hawkins† approaches the closest to nature.

The under part of the basi-occipital expands and terminates anteriorly in a pretty regular curve, with the convexity directed forwards. There is in some species a slight emargination in the middle of this curvature entering into the formation of the circumference of the Eustachian outlet of the basi-sphenoid.

The articular tubercle of the basi-occipital frequently presents

* *Geol. Trans.*, 1822, p. 117. † Memoirs on *Ichthyosauri*, fol. Pl. I.

near its middle a vertical depression, as if for the insertion of some ligament.

I may here observe that in true Fishes the concave articular surface is present on the body of the posterior cranial vertebra, or occiput, as it is on the bodies of the ordinary vertebræ. The deviation from this character in the *Ichthyosaurus*, and the substitution of a diametrically contrary structure, bespeaks strongly its true Saurian nature. In the *Cetacea* the basi-occipital forms no part of the articulation with the vertebral column, but the head is joined to the atlas by two ex-occipital condyles as in other *Mammalia*.

The ex-occipitals are proportionally smaller in the *Ichthyosauri* than in the Crocodiles, and do not unite together so as to complete the boundary of the foramen magnum above, but allow the supra-occipital element to form about one third of the upper circumference of this foramen. This approximation to the Lacertian type, of which the discerning eye of Mr. Conybeare* had led him to entertain a suspicion from mutilated specimens, I have ascertained beyond doubt to be a generic structure in the *Ichthyosauri*.

Two very strong mastoid bones extend from the ex-occipitals towards the articular extremity of the tympanic bone, and nearly obliterate the space intervening at the back part of the skull between the parietal bifurcations and the occipital bone.

The solid structure of the back part of the cranium which thus ensues gives to the skull of the *Ichthyosaurus* a strong resemblance to that of the Crocodile; but as this is an adaptive rather than a typical conformation, it affords but a slender argument for their affinity. The development of the occipital bones in both cases depends on the necessity for a due extent of surface for the implantation of the powerful nuchal muscles which must have mainly wielded a head produced anteriorly into long and heavy jaws beset with numerous and formidable teeth.

The upper part of the cranium includes the parietals, the composite frontals, and the principal elements of the temporal bones.

The parietals form together a strong triradiate bone, as in the Plesiosaurs. The temporal muscles, which derive part of their origin from its median and anterior portion, extend to the middle line, where they are separated by an osseous intermuscular crest. Anterior to this crest, close to or in the coronal suture, the parietal is perforated,—a structure not present in the Crocodiles, but peculiarly characteristic of the Lacertian Saurians, and the

* *Geol. Trans.*, 1822, p. 117.

more important as an indication of affinity, because it is not an adaptive character. The two posterior and symmetrical processes of the parietal extend outwards to abut against the tympanic and squamous bones, and give additional strength to the point of resistance against which the lower jaw works.

The inner surface of the median parietals is not, as might have been anticipated, in immediate contact with the cerebral membranes, but rests upon a symmetrical median single plate of bone of a subquadrate form, with the posterior angles thickened and supporting two surfaces which articulate more immediately with the superincumbent parietals. The internal superficies is concave, the external convex with two vascular foramina in the same transverse line. This bone I take to be the interparietal, overlapped by the ordinary lateral parietals, which are anchylosed together.

The temporal aperture is circumscribed by the jugal, zygomatic, and tympanic bones, and is reduced to much smaller dimensions than in the Lacertians, owing in part to the greater breadth of the zygomatic element of the temporal. In the Marine Tortoises the whole of the temporal aperture is concealed by a continuous ossification extended from the parietals and posterior frontals to the zygomatic arch.

In the Crocodiles we find a part of this structure still remaining in the osseous bridge which traverses longitudinally the temporal fossa between the parietal and the posterior frontal bones.

In the Lacertians and Plesiosaurs the temporal fossa is single on each side, but in the *Ichthyosaurus* we find a transitional structure in the occurrence of a second distinct fossa in the temporal region left between the zygomatic and tympanic bones.

In the general position and strong and immoveable condition of the principal bones forming the pedicle for the articulation of the lower jaw, there occurs, as might be expected, a deviation from the Lacertian type, and a similarity to that higher Saurian family, in which there is a similarly ponderous and well-armed lower jaw.

The tympanic or articular bone, instead of being attached only by its upper extremity to the conjoined squamous and mastoid elements of the temporal, is in the Ichthyosaur wedged in between the mastoid, squamous, and zygomatic elements, and is further established in this position by the irregularly dentate structure of the sutures.

This bone, moreover, presents an unusual degree of solidity and robustness in all the species of *Ichthyosaurus*. It thus

affords a strong and unyielding point of resistance for the movements of the lower jaw,—an adaptation which the size, armature, and violent uses of that jaw in predatory attacks rendered indispensable. And as a similar structure and office of the lower jaw of the Crocodile is the condition of a corresponding strength and fixation of its articular pedicle, so in this part of the cranial structure we find that the *Ichthyosaurus* resembles the Crocodilian and differs from the Lacertian types of structure.

There is still another peculiarity of the articular pedicle of the lower jaw of the *Ichthyosaurus*, for the intelligibility of which it will be necessary to premise some observations on the structure of the same part in the existing oviparous *Vertebrata*.

Commencing with the structure of the articular pedicle of the lower jaw in the bird, we shall find that the tympanic bone is connected with the upper jaw by two osseous columns, of which the lower one abuts against the palatal bones, while the second and superior extends to the lower angle of the superior maxillary bone. It is by means of these two columns that the movements of the articular pedicle are communicated to the upper mandible.

It is with the upper of these osseous columns, which relates to the Ichthyosaurian structure under consideration, that we are at present concerned. Its usual appearance is that of a simple osseous style, and it is described by Cuvier and other comparative anatomists as the analogue of the jugal bone. If, however, the state of this apparently simple style be looked into in the embryo bird, it will be found to consist of two distinct parts,—that ossification commences by two distinct centres. In the Ostrich, indeed, which is one of the most reptilian of birds, the two bones remain distinct to nearly the period of full growth.

The anterior of these I regard as the true *os jugale*; the posterior as the homologue of the *os zygomaticum*, otherwise entirely wanting in the skull of the bird.

Now here the important point gained in tracing the homologies of the lateral bones of the skull through the Saurian group is the evidence of the separate existence of the squamous and zygomatic elements of the temporal bone in the same cranium. In the Lacertians the zygomatic, squamous and jugal bones are always distinct; but the zygomatic style extends to the proximal instead of to the distal end of the *os tympanicum* as in Birds. Its position, parallel with the malar bone, is the same however as in Birds; and, as in that class, the anterior extremity of the zygomatic bone is joined to the malar bone, and is directly continued from it.

In the Crocodilians the position of the os zygomaticum is

altered; its anterior extremity abuts against, and is confluent with the squamous element of the temporal bone, whilst its opposite extremity is wedged in between the tympanic and the jugal bones; the whole of the posterior margin of the os tympanicum moreover runs parallel with, and is firmly united to the os tympanicum.

In the *Ichthyosaurus* the os zygomaticum is present as a separate bone, and resembles in its massive proportions that of the Crocodile: the anterior extremity is expanded; of this the greater part is articulated with the posterior extremity of the jugal bone, and the remainder with the squamous element of the temporal. The opposite end of the os zygomaticum abuts, as in the Bird and Crocodile, against the lower or articular extremity of the os tympanicum, but without having the whole of its posterior margin united with the tympanic bone, as in the Crocodile; hence results that vacancy which Mr. Conybeare* has termed the "lower temporal fossa," and which he describes as being bounded below by "another bone interposed between the os quadratum and the jugal," and considered by him "as another dismemberment of the temporal." The true homology of this bone could not be appreciated whilst the squamous element of the temporal bone was regarded as including also the zygomatic, or as a "squamoso-zygomatic bone"; but when the independent origin of the zygomatic bone has been determined, and its modifications traced through the existing Saurian types, the precise nature of the dismemberment of the temporal which plays so conspicuous a part in the articulation of the lower jaw of the *Ichthyosaurus* is at once recognisable. In the strength of the zygomatic bone, and its connection with the articular inferior end of the tympanic we perceive the Crocodilian character, while in the free circumference of the zygomatic bone we find them associated with Lacertian peculiarities. The *Ichthyosaurus* thus offers a beautiful transitional structure between the two great existing modifications of the Saurian types which we should in vain look for elsewhere.

The peculiarly large orbital cavity in the *Ichthyosaurus* includes in its circumference six distinct bones: above, the anterior, median and posterior frontals; in front, the large lachrymal bone; below and behind, the jugal and apparently a distinct and peculiar posterior bone.

In the separation and relative position of the median and anterior and posterior frontals the cranium of the *Ichthyosaurus* accords with the usual Saurian characters; but these bones are

* *Additional Notices*, p. 114.

relatively larger than in the recent Sauria. This increase of development of the anterior and posterior frontals is dependent on the large size of the eye and the cavity destined to contain it.

In the composition of the facial part of the skull and the relative sizes and disposition of the bones forming the nasal cavities and upper jaw, we have the same beautiful examples of a transitional structure between the Crocodilian and Lacertian types of structure as have been noticed in other parts of the cranium. With respect to the nasal apertures the tendency is mainly towards the Lacertian structure, coupled with peculiarities purely Ichthyosaurian.

In the Monitors for example, the bony external nostrils commence at the upper part of the cranium, at a very slight distance in front of the orbit; but they extend to near the anterior point of the upper jaw, where they are bounded by the turbinated bones. The rest of their circumference is due to the nasal, intermaxillary and superior maxillary bones.

In the Crocodile, as is well known, the nostrils are placed at the anterior part of the face, and are bounded by the nasal and intermaxillary bones. In the *Ichthyosaurus* the nostrils are limited to the position at which they commence in the Monitors, viz. at a short distance anterior to the orbits; and nearly the whole of their posterior circumference is due to the lachrymal bones, which do not at all enter into the composition of the external nostrils in the existing Saurians.

The characteristic structure and position of the external nostrils in the *Ichthyosauri* dubiously hinted at by Home, were afterwards admirably determined by Mr. Conybeare.

The upper maxillary bones are remarkable in the *Ichthyosauri* for their small size; they contain rarely more than the posterior third part of the dental series of their own side. In the Crocodiles the superior maxillary bones have a much greater relative extent, and contain generally three fourths of the dental series. The relative size of the maxillary bones is still greater in the Lacertians. It is in Fishes that we find the nearest resemblance to the *Ichthyosauri* in the comparatively insignificant share which the superior maxillary bones contribute to the formation of the dentigerous margin of the upper jaw. The intermaxillary bones on the other hand present in the *Ichthyosaurus* as peculiar a degree of superior magnitude; a difference, however, which does not so much arise from the prolonged form of the snout, as from the disproportionate shortness of the maxillary bones.

When we compare for example the jaws of the *Ich. tenui-*

rostris with those of the Gangetic Gharrial, an equal degree of strength and secure attachment of the teeth seem to result from the two very different proportions in which the maxillary and intermaxillary bones are combined together to form the upper jaw. The prolongation of the snout has evidently no relation to this difference; and we are accordingly led to look for some other explanation of the disproportionate development of the intermaxillary bones in the *Ichthyosaurus*. It appears to me to give additional proof of the collective tendency of the affinities of the *Ichthyosaurus* to the Lacertian type of structure. Its aquatic habits necessitated the peculiar position of the nostrils, and their limited extent in that position. But in the Lacertians, in which they extend to the fore part of the head, their interior boundary is formed by the intermaxillary bones; these bones, therefore, conformably with the laws of organic combinations, are extended backwards in an unusual degree, in the *Ichthyosaurus*, to enter into their ordinary relations with the nasal apertures, which are situated unusually far back in the head. Before quitting the present subject I may remark that in the Lacertians the median suture of the intermaxillary bones is soon obliterated; while in the Ichthyosaur it is persistent as in the Crocodile; but this is a circumstance of minor importance. The nasal bones in the Ichthyosaur differ from those of both the Crocodilians and Lacertians in having no connexion with the maxillary bones.

In considering the conformation of the base of the cranium I shall proceed in continuation to describe that part which enters into the structure of the upper jaw, as these have been less accurately described than any other part of the skull.

The intermaxillary bones constitute a considerably greater extent of the osseous palate than in either the Crocodilians or Lacertians. They are not perforated as in the Crocodile, but are simply emarginate on the outer side of the posterior part of their palatal processes, which form, in connection with a corresponding emargination of the palatal processes of the maxillary bones, the palatal foramina.

The maxillary bones constitute a comparatively small part of the bony palate, and are, according to my observations, separated widely by the intervening palatal bones and vomer, thus resembling rather the Lacertian than the Crocodilian type.

The palatal bones are joined together by a median suture, except where the wedge-shaped anterior extremities of the pterygoid bones are inserted into their posterior interspace, the pterygoids, in like manner, being separated from each other posteriorly by the intervening body of the sphenoid. The

posterior apertures of the nostrils are thus thrown far back, as in the Crocodiles.

The transverse bones, or external pterygoids, complete the boundaries of an aperture between the pterygoid and maxillary bones; and by abutting against the posterior extremities of the maxillary bones greatly increase their strength.

It appears to me, from a close inspection of some of the most complete specimens of this intricate part of the skeleton of the *Ichthyosaurus* in different museums, that the posterior nostrils are not perforated, as in the Crocodile, exclusively in the pterygoids, but that they occur in the interspace between the internal pterygoids and the basi-sphenoid, as in the Lacertians. With respect to the sphenoid, however, there is a structure characteristic of the Crocodiles, or at least not present in those Lacertian crania which I have had opportunities of examining, viz. an oblique perforation of the basi-sphenoid for the passage of the common termination of the Eustachian tubes. The contour of the basi-sphenoid is heptagonal: the posterior margin is the broadest, and is articulated by a thick rough surface, with a corresponding margin of the basi-occipital. The oval petrous bones are articulated to the sides of the sphenoid; each of the anterior lateral angles are produced, but not to the same relative extent as in the Lacertians, in which they extend as buttresses to the internal pterygoids: a moderately long median slender pointed process is continued forwards from the middle of the anterior surface of the sphenoid. The superior or cranial surface of the sphenoid is traversed by a transverse ridge.

With reference to the lower jaw, it would be superfluous to offer any observations after the admirable and accurate exposition of its composite structure which has been given in the works of Conybeare, Cuvier, and Buckland. I shall therefore limit myself to a comparison of its leading features with the peculiarities of the two great divisions of the existing Saurians.

The dentary piece resembles that of the Lacertians and differs from that of the Crocodilians in being pierced externally by a few large vascular foramina disposed in a regular series. It differs also from the Crocodilian type in terminating posteriorly *above* instead of *beneath* the anterior extremity of the sur-angular piece. In the degree of development of the coronoid or complementary element the lower jaw of the *Ichthyosaurus* holds an intermediate place between the Crocodilian and Lacertian groups; it is of greater extent than in the Crocodile, especially posteriorly, but does not send upwards a well-defined process, as in the Lacertians. The process analogous to the

coronoid is more developed than in the Crocodiles, but appertains, as in them, to the surangular element. The jaw of the recent Crocodilians is characterized by a large vacancy which occurs between the angular, surangular, and dentary pieces, while the surangular itself is imperforate. In the Lacertians no vacancy occurs between the above-named maxillary elements, but in some genera, as the *Iguana*, the surangular is perforated. Now, in this highly characteristic deviation from the Crocodilian structure, we find the *Ichthyosaurus* participating with the inferior or Lacertian Saurians; and in some species, as the *Ich. communis*, the surangular bone exhibits a well-marked perforation. It is interesting to observe that in the *Teleosaurus* and *Steneosaurus* the vacuity between the angular and surangular pieces is reduced to a very small size, which, in combination with the modification of their vertebral column is evidence of their transitional character between the great carnivorous Saurians of the past and present epochs. In the backward extension of the surangular bone the *Ichthyosaurus* manifests an affinity, not exclusively to the Crocodiles, but to certain Lizards also, as the large Monitors, in which there is a corresponding development of the surangular bone in that direction. In the depth of this piece, on the other hand, the *Ichthyosaurus* strikingly resembles the Lacertian and deviates from the Crocodilian type. The conformation of the posterior angle, the robustness of the articular extremity of the lower jaw, in short, all those characters that relate to the muscular forces destined to wield an instrument armed with numerous and large destructive teeth, approximate the Ichthyosaurian jaw more nearly to the Crocodiles than to the feeble Lizards; but in those characters which more truly indicate affinities to a typical structure, as being less liable to modification for particular functions, the *Ichthyosaurus* decidedly manifests its closer affinity to the Lacertian character.

The intermediate or annectent characters of the *Ichthyosaurus* between the Crocodiles and Lizards is exemplified in a remarkable degree in the modification of that part of both the upper and lower jaws which is destined for the support of the teeth.

The *Plesiosaurus*, like the Crocodile, has its teeth lodged in distinct sockets: the Lacertian Sauria have their teeth anchylosed, like the teeth of most fishes, to the alveolar process of the jaws, which process is a simple plate corresponding to the outer wall of the alveoli in the higher Reptiles; the inner alveolar plate being very slightly, if at all, developed. In the *Ichthyosaurus* both the outer and inner plates of the alveolar groove are present, and the teeth have their bases free, as in the Crocodiles,

but they are not lodged in distinct sockets formed by the development of bony partitions in this groove.

The base of the teeth of the *Ichthyosaurus* is, however, covered with a layer of cementum or true bone, which makes the anchylosis of such part to the contiguous jaw quite possible; as it is through the medium of a like investment that anchylosis actually does take place in the existing Lacertians. The pulp of the tooth of the *Ichthyosaurus*, after it has been subservient to the development of the crown of the tooth, becomes solidified in the fang or base by a coarse ossification which closes the pulp-cavity at the lower part. Mr. Conybeare, who first pointed out this fact, at the same time indicated the difference thereby illustrated between the Ichthyosaur and Crocodile. But the non-existence of anchylosis of the teeth to the jaw, and the development of the inner wall of the alveolar groove, together with the slight ridges intervening between the teeth, all tend, Mr. Conybeare observes, to place the *Ichthyosaurus* much nearer the Crocodilian than the Lacertian division of the existing Saurians.

Hyoidean Arch.—In three examples of the *Ichthyosaurus*, Cuvier* detected the two horns or stylo-hyoid elements of the hyoidean arch in their natural situation; and he also states, that he had seen between, and in advance of these lateral elements, an osseous disk, broader than long, notched posteriorly, and which he suspected to be the body of the os hyoides. These—the only elements of the hyoidean system which are present in the skeleton of the *Ichthyosaurus*—are beautifully displayed in their natural relative position in the *Ichthyosaurus lonchiodon* in Mr. Hawkins's second collection, now transferred to the British Museum.

The cornua are robust, elongated, sub-prismatic bones, slightly enlarged and truncate at both extremities: their junction with the small flattened hyoid body seems to have been by means of abundant flexible ligamentous material: the length of each cornu is a fifth part the length of the lower jaw.

The condition of the hyoid apparatus is of great weight in reference to the habits and affinities of the extinct animals in question; for in fishes, and the water-breathing reptiles, this apparatus presents a magnitude and complexity proportionate to its importance as the foundation of the branchial system.

In the Lacertian Sauria, the os hyoides, though less complicate than in Fishes, presents characteristic modifications in the number and length of the lateral appendages which relate to

* Ossem. Fossiles, tom. v. part xi., De l'Ichthyosaurus, Art. iv., De l'Os Hyoide.

the size and uses of the thick or long, and commonly bifurcate tongue; while in the Crocodile the hyoid apparatus is reduced to the same number of pieces as in the *Ichthyosaurus*, the body or median plate, however, being cartilaginous, and the two straight cornua relatively smaller. This simple apparatus is far less subservient to the support or movement of the tongue,—which, as Aristotle long ago pointed out, is as little conspicuous in the Crocodile as in many fishes,—than to the mechanism for defending the larynx and pharynx from the entry of water, during the struggles of a submerged prey, when the mouth of the air-breathing destroyer is necessarily exposed to the free ingress of the ambient element. The condition of the hyoid apparatus in the *Ichthyosaurus*, besides corroborating the evidence afforded by the rest of the skeleton that this extinct reptile was an air-breather, indicates that its tongue was almost as little developed as in the Crocodile; and since the *Ichthyosaurus* obtained its food at all times under the same circumstances which necessitate the modification of the hyoid apparatus in the Crocodile, it may be inferred that the hyoid arch was physiologically related to the working of a similar valvular apparatus for defending the orifice of its air-tube from the water admitted into the interspace of the jaws during the capture and slaughter of its prey; and the structure and relative position of the hyoid apparatus corroborates this inference.

Vertebral Column.—In the vertebræ of the *Ichthyosaurus* are observed the centrum, the neurapophyses and their spine, the hæmapophyses, and the costal elements. The centrum or vertebral body is characterized, as is well known, by its antero-posterior compression and the concave form of its articular surfaces, a structure in which the *Ichthyosaurus* departs from the Crocodilian and Lacertian types of *Sauria*, and resembles the Perennibranchiate Amphibians and Fishes. The body of an Ichthyosaurian vertebra might however be distinguished from that of any fish, by the presence of the neurapophyseal pits on each side of the shallow canal for the spinal cord; for the neurapophyses, though anchylosed above to each other and to their spinous processes, always remain detached from the centre below. We cannot attach much force to the teleological argument founded upon this structure, or admit its necessity to co-operate with the cupped form of the intervertebral joints in giving flexibility to the vertebral column, and assisting its vibratory motions necessary in the mode of progression, which seems to have been common to the *Ichthyosaurus* and Fishes; because in all the osseous fishes these parts are consolidated with the vertebral centrum as in the Cetaceans and other Mammals, and yet the vertebral column is not so locked together as to render impossible such motion of its

parts as is requisite for swimming. In the separate state of the neurapophyses in the Ichthyosaurs we perceive a condition which is essentially Saurian, and one which doubtless would add somewhat to the facility of inflecting the spine in the vertical directions.

The neurapophyses are interlocked together by means of coadapted oblique processes. The hæmapophyses are developed beneath the abdominal and the caudal vertebræ; they always remain distinct from or unanchylosed to the vertebræ above, and, so far as I have been able to form an opinion, are not united together below, or to a common spine.

With respect to the structure of the anterior part of the cervical region of the vertebral column, different views may be entertained. One theory is as follows: The atlas and axis resemble each other and the succeeding vertebræ in size and general form, as is the case in Fishes; but they are anchylosed together, and the united surfaces are plain, presenting the only deviation from the characteristic cupped structure recognisable in the rest of the vertebral column. Mr. Conybeare observes, "We have only seen the inferior piece or body, if it can be so called, of the atlas, and the odontoid process (which in all reptiles forms a distinct piece) of the axis: they very nearly resemble those of the Turtle." It is not improbable that one or other of the subvertebral wedge-bones discovered and well described by Sir Philip Egerton may here be alluded to; but Mr. Conybeare afterwards believed that he had been deceived by a mutilation of the occipital condyle.

Comparative anatomists are not agreed as to the exact nature or signification of the odontoid process; its ossification always begins by a distinct centre in the Mammalia, in which class it becomes anchylosed with the body of the axis; and it is generally regarded as a peculiar epiphysial appendage to the central element of the 2nd cervical vertebra. According to this view, the three subvertebral wedge-bones, which Sir P. Egerton has so satisfactorily and perseveringly traced out, may be regarded as analogous epiphysial appendages of the first three cervical vertebræ. Or they may be viewed, with the odontoid process, as hæmapophyses or inferior appendages of the vertebral centres in a rudimental state; and the vertebral cup which receives the occipital tubercle may be deemed, as in fishes, to be formed by the atlas.

On the other hand, if we look to the Saurians for a clue to the homologies of the structure in question, we find that the body of the atlas in both the Crocodilian and Chelonian Reptiles is always remarkably small, and the greater part of the articular concavity which is adapted to the occipital tubercle is

formed by the odontoid epiphysis of the axis, which nearly equals the body of the same vertebra in size. In the Lacertians, the body of the atlas is a thin annular piece of bone, which forms merely the circumference of the articular cup for the occipital tubercle; the greater part of which articulates, as in the Crocodiles and Turtles, with the odontoid epiphysis of the axis. The consideration of these facts leads to the suggestion of another hypothesis of the analogies of the anterior vertebræ in the *Ichthyosaurus*. What has been described as the atlas anchylosed with the axis, may be the true odontoid process of the axis exhibiting the same excessive development and anterior concavity adapted to the occiput, as in the Lacertian Sauria. The first subvertebral wedge-bone, according to that view, would then represent the body of the atlas, as Mr. Conybeare indeed seems to have regarded it, but reduced to a still more atrophied condition than in the Crocodile or Turtle. But here another difficulty presents itself: admitting the first subvertebral bone to be the atrophied body of the atlas, what then it may be asked are the second and third subvertebral bones? I confess that in this, as in some other of the problems of morphology, I see only a choice of hypotheses of which none are free from objection. This at least is certain, that the subvertebral, cervical, wedge-shaped ossicles, which hitherto have been observed only in the *Ichthyosaurus*, are most admirably adapted, as Sir Philip Egerton has well pointed out, with the anchylosed condition of the atlas and axis, to ensure the fixation of the head which is essential, in an active predatory animal, to its swift and agile movements through the water.

The costal processes or ribs commence in the *Ichthyosaurus* at the axis or second cervical vertebra, and are continued through the anterior two thirds of the caudal region of the spine. Those of the cervical and anterior part of the thoracic regions are slightly bifurcate at their proximal extremity, and are articulated partly with a tubercle on the centrum, which represents the inferior transverse process, and partly to the outside of the base of the neurapophysis: as they become placed further back, the two heads become gradually blended into a single expanded proximal extremity, which at length becomes a simple convex tubercle in the ribs of the caudal region. The ribs quickly increase in length, which is greatest at the middle of the thoracic abdominal cavity: from this point they become gradually abbreviated to the sacral vertebræ, and then suddenly contract into short and straight appendages, which progressively diminish until they finally disappear. Those ribs, which are bifurcate or bilobed at their upper extremity, are traversed by a longitudinal impression extending from the angle of bifurcation along

the whole of the anterior and posterior surfaces, giving to the bone the appearance of its being composed of two ribs anchylosed by their sides. Mr. Clift has given a faithful view of this structure in Pl. XIX. of the Philosophical Transactions for 1814. It is gradually lost in the ribs at the posterior part of the thoracic-abdominal cavity, which from this part preserve the form of simple osseous styles. The inferior or sternal extremities of the opposite ribs are not immediately united together, but the long hoop is completed by a sterno-costal arc composed of transverse styles, which are more slender and fewer in number than in the *Plesiosaurus*.

In comparing the vertebræ in different parts of the spine of the *Ichthyosaurus*, modifications of structure present themselves which are somewhat analogous, though minor in degree, to those already described in the *Plesiosaurus*. The principal difference to be noticed is that the lower tubercle for the attachment of the rib never wholly quits the centrum: any detached vertebral centrum, therefore, that might be discovered, which had no lateral tubercle or articular surface for a rib, might be safely pronounced, whatever the form of its anterior and posterior articular surfaces, not to have belonged to a true *Ichthyosaurus*, provided it was not compressed laterally, as in the small terminal ribless caudal vertebræ which supported the caudal fin in the *Ichthyosaurus*.

In the anterior sixteen vertebræ of the *Ichthyosaurus communis*, or for a third part of the spine extending between the cranium and pelvis, the lower costal tubercle only is developed upon the body, the upper tubercle or articular surface resting on the neurapophysis, or not being distinct from the neurapophysial articular surface. In the twenty succeeding vertebræ, both the costal tubercles are developed on the side of the centrum below the neurapophysial depression. The upper of these tubercles is at first placed close to the neurapophysial pit, and thence takes gradually a lower position on the side, so as to approach more nearly to the inferior tubercle; at length near the 40th vertebra, at a short distance beyond the iliac bones, the two tubercles blend together and form a single ridge. This ridge as the caudal vertebræ recede from the trunk gradually changes its obliquely elongated direction for a transverse one, or becomes a rounded tubercle; and at length disappears about the 80th vertebra. It is at this part of the spine in the *Ichthyosauri communis* and *intermedius* that the abrupt bend or dislocation of the tail usually takes place; and here three or four of the vertebral centres are more compressed than those which immediately precede or follow them, and their margins are raised, as if by forcible compression. The caudal vertebræ are more

especially characterized by a small triangular tubercle developed at each of the four angles of the quadrilateral space which forms the inferior surface, the tubercles being largest at the anterior angles: this tubercle, with the corresponding one on the adjoining vertebra, forms the surface of articulation for the caudal hæmapophyses, which are short and slightly curved simple bones, not joined together at their distal extremities so as to constitute a bone of the chevron shape, as in the existing Saurians: these hæmapophyses disappear in the last 20 vertebræ.

Besides their double-cupped articular surfaces and the presence of the costal tubercle, the vertebral centres of the *Ichthyosauri* generally differ from those of the *Plesiosauri* in having a more angular contour, which sometimes forms nearly a true hexahedron.

Each sterno-costal arc consists, as Mr. Hawkins correctly states*, of five bones. These are slender transversely elongated ossicles, which overlap each other in the way which has been described in the *Plesiosaurus*. The median piece is generally symmetrical in shape, and sends off from its middle part a short thick process both forwards and backwards, or in the longitudinal axis of the body. In the symmetrical median pieces, the elongated lateral processes are continued from the middle of the short longitudinal one; in the unsymmetrical median pieces one lateral process comes off from the anterior and the other from the posterior portion of the longitudinal process. The lateral processes bend slightly backwards, and diminish to a point. A slender cylindrical styliform process pointed at both extremities is spliced as it were to the anterior part of each lateral process of the median piece, which it equals in length; it extends however a short distance beyond its lateral extremity. A second styliform ossicle is similarly adapted to the anterior part of the lateral or outer extremity of the preceding piece, but the lateral extremity of the third process is not pointed but is slightly expanded and abruptly truncated in order to join the lower extremity of the true rib†. In the singleness of the median piece and the development from its middle part of a short longitudinal process may be discerned an affinity to certain Lacertian Sauria, as the *Polychrus marmoratus*, Cuv., while in the double overlapping lateral pieces we perceive a resemblance to the condition of the abdominal ribs in the Crocodile. If the median piece in the *Ichthyosaurus* were removed,

* Memoirs on *Ichthyosauri*, p. 21.

† In a rare and beautiful specimen of this sterno-costal apparatus of a species of *Ichthyosaurus* from the lias of Lyme Regis, kindly transmitted to me for examination by Mr. Conybeare, there are 19 slender arcs, each composed of the five pieces above described.

the sterno-costal arc would be reduced to the same number of elements as in the Crocodile: if the lateral styles were taken away, it would resemble the simple sterno-costal arc of the *Polychrus.*

The Pectoral Extremity.—We have already remarked that the extremities of the *Enaliosauri* offer the nearest resemblance in their bony structure to the paddles of the *Cetacea.* But this resemblance is limited to the radiated system of bones, i. e. to the *brachium, antibrachium* and *manus.* The mode in which the locomotive member is connected with the trunk is entirely different in the two aquatic tribes. In the *Cetacea* the pectoral fin is attached to a simple scapula with a rudimental acromial and coracoid process, and is merely suspended in the flesh. In the *Ichthyosaurus,* as in the *Plesiosaurus,* the pectoral fin is connected with, and must have acted upon a powerful and resisting osseous arch, having the sternum for its keystone. The sternum in fact here exists solely for the function of the anterior members, and does not enter at all into the formation of the costal arches or the respiratory cavity. In the *Cetacea* on the contrary the sternum is limited to its connection with the ribs, and to the completion of the thoracic receptacle of the large and highly developed lungs.

In the *Ichthyosaurus* the representative of the sternum is analogous to the episternal element as it exists in the *Ornithorhynchus* and Lacertian *Sauria,* and, as in many of the latter tribe, it presents a triradiate form. One branch occupies the median line of the pectoral arch, is broad and flat, and rounded posteriorly; the other two rays branch off from each of the anterior angles of the median piece, and, diverging laterally, follow the curvature of the superimposed clavicles, to the posterior and middle part of which they are closely atached: as they proceed outwards, these lateral rays of the *episternal* bone gradually diminish to a point.

The scapula is a short but stout and broad bone presenting the simple parallelogramical form which characterizes it in the *Oviparous Vertebrata.* The anterior margin is fixed to the clavicle and to the extremity of the lateral process of the episternum: the inferior extremity presents two facets, one of which is attached to the coracoid bone, the other forms part of the articular surface for the humerus.

The coracoid bones, which constitute at their contracted and thickened outer extremities the remainder of the glenoid cavity, become suddenly and remarkably thinner and expanded as they pass inwards to articulate with the episternal bone. They are also complicated each in the young *Ichthyosauri* with an epiphysial piece wedged into the angle between their ante-

rior margins and the episternum, which pieces correspond with the epicoracoids of the Lacertian *Sauria* and *Ornithorhynchus*. The existence of these bones I have determined in some of the beautifully worked out skeletons in the collection of Mr. Hawkins.

The clavicles are strong, elongated, slightly curved bones, thicker in the middle than at their extremities, articulated by an oblique suture to the transverse processes of the episternum with their median extremities in contact, but not anchylosed together as in the furculum of the Bird: in this respect, as in their connection with the episternal bone, they correspond with the clavicles of the *Ornithorhynchus*. In the entire mechanism of the complex pectoral arch, indeed, the resemblance between these very different animals is remarkably close, as Mr. Clift first pointed out, while the difference which both these air-breathing aquatic animals present in this part of their osseous structure from the *Cetacea* is very striking. In the *Cetacea*, for example, there is not any osseous bar interposed between the two shoulder-joints, or the centres on which the fore paddle worked, while similar movements of the fore paddles of the *Ichthyosaurus* had, and in the *Ornithorhynchus* have their momentum transferred to, and resisted by, not less than three transverse bones, viz. 1st by the clavicles, 2nd by the episternal forks and the scapulæ, and 3rdly by the coracoids and scapulæ. To what difference in the habits of these species had these differences of structure reference? Most assuredly it could not relate exclusively to the necessity of rising to the surface to respire air, as conjectured by Sir Everard Home*; for this necessity existed in all the three types of aquatic animals, and much more imperatively in the *Cetacea* than in the *Enaliosauria*. In the *Ornithorhynchus* the anterior extremities are directed outwards, as in the marine *Cetacea* and *Enaliosauria*; but they are destined in that quadruped to be applied not only to displace water, but to be occasionally pushed against a more resisting surface, as the dry land: in order therefore to enable the fore limbs to react with due force upon the body, a strong apparatus of bone is introduced between the two shoulder-joints, whereby these parts are prevented from yielding inwards and compressing the soft muscular masses. But in the *Cetacea*, which were never intended to quit the deep, such an apparatus of bone, as it would have added unnecessarily to their weight, has been excluded from the mechanism of their anterior extremities: and hence it is that, when they have the misfortune to be stranded, they are unable to regain their native element. The instrument

* Phil. Trans., 1818.

for bringing the head to the surface of the water for the purpose of breathing is the same in both the Monotreme and the Cetacean, viz. a strong, muscular, horizontally flattened tail. In the *Ichthyosaurus* a pair of hinder paddles (which in the large-headed species, as the *Ich. platyodon*, are equal in size with the fore paddles,) must have fully compensated for that different construction of the tail, which, while it rendered it less efficient as a means of raising the head to the surface, made it a more perfect instrument in ordinary natation; and the sufficiency of this compensation will be better appreciated when it is remembered that the Reptilian structure of the lungs and heart of the *Ichthyosaurus* would allow it to dispense with so perfect a machinery for rising to the surface as was essential to the warm-blooded aquatic species above cited.

For what purpose then were sterno-clavicular and coracoid arches assigned to the *Ichthyosaurus*? Doubtless that the anterior paddles might be subservient to locomotion not only in the water but on land; and that, when applied to the resisting soil, they might react with due force upon the trunk. It is very conceivable that the *Ichthyosaurus* like the Crocodile may have come ashore to sleep: it is most probable that they resorted to the shore to deposit their eggs, supposing them to have been oviparous, as the sum of the analogies deducible from their osseous texture would indicate. The hind paddles would also be serviceable in terrestrial progression as in the *Ornithorhynchus*, while in the strictly marine *Cetacea* they could readily be dispensed with.

The radiated bones of the anterior extremities consist of a distinct humerus, radius and ulna, carpal, metacarpal, and phalangial bones.

Both the brachial and antibrachial bones of the *Ichthyosaurus* are much shorter and broader in proportion to their length than are the corresponding bones of the *Plesiosaurus*. This is more particularly the case with the radius and ulna, which are usually broader than they are long, and closely resemble the carpal bones which succeed them. The limits of the carpus can be by no means so easily defined as those of the antibrachium. The first row of bones which succeeds to the radius and ulna includes three polygonal or rounded flat bones, generally broader than they are long; the next row includes three or four similar bones; and then instead of being succeeded by elongated metacarpals and phalanges, as in the *Plesiosauri*, the fin is supported by numerous rows of smaller but similarly-shaped flattened ossicles, increasing in number as they diminish in size, to near the extremity of the paddle. These small flattened ossicles are arranged in from three to six digital series, and are generally dovetailed

into each other at the sides, so as to prevent any independent movement, but to constitute one uniformly resisting framework of a powerful fin. The integument extended beyond the bones further than might have been anticipated, and its posterior margin appears to have been supported by a series of slender bifurcate cartilaginous rays. A disparity in the size of the anterior and posterior extremities has usually been assigned as a generic structure or character of the *Ichthyosaurus*; but in the species in which the head is unusually large, as the *Ich. platyodon*, the pelvic extremities are as large as the pectoral ones. In most of the known species, however, the hinder paddles are much smaller than the fore ones. In all they present a structure which is closely analogous to that above described.

The Pelvic Extremity.—The pelvic arch consists, as in the *Plesiosaurus*, of an ilium, ischium and pubis on each side. The ilium is a short, simple, strong, compressed bone, slightly expanding as it descends to combine with the ischium and pubis in forming the acetabulum. Its upper and proximal extremity is not connected by synchondrosis to the extremities of the sacral ribs, but lies simply upon them, just as the scapula rests upon the ribs at the anterior part of the thorax. This is a condition of the ilium which is of great interest, and is peculiarly characteristic of the *Enaliosauria* among Reptiles*. It renders their pelvic extremities remarkably analogous to the ventral fins of fishes, which are in like manner simply suspended in the muscular mass and not fixed to a sacrum. The ischia and pubes are both relatively much smaller than in the *Plesiosaurus*; the pubis is slightly expanded at its mesial or lower end, but the ischium is a simple, elongated, slightly compressed bone.

The femur is usually longer in proportion to its breadth than the humerus, and its posterior margin is more concave. It is succeeded by two bones, which represent the tibia and fibula, and resemble in their forms and proportion the radius and ulna of the fore fin. Three irregular polygonal flattened bones succeed the tibia and fibula; and then three or four longitudinal series of similar bones follow, which gradually diminish in size as they approach the extremity.

Such appears to be the structure of the skeleton which is common to all the species of *Ichthyosauri*, so far as I have been able to study their fossil remains, and which may, therefore, be considered to characterize them generically.

The modifications of this structure which distinguish the particular species next claim attention.

* The rudimental pelvic extremities of Serpents are simply suspended in the muscles external to the adjoining ribs, as were those of the *Ichthyosaurus*.

Ichthyosaurus communis, Conybeare.

This species is characterized by its relatively large teeth, with expanded or ventricose bases, and round, conical, slightly aduncate crowns, which taper more quickly and less regularly to the apex than in the other species; the apex is not very acute, and the whole tooth is longitudinally furrowed, the base being sculptured by coarse and deep grooves, with intervening convex ridges. The upper jaw contains on each side from 40 to 50 teeth, of which 18 are implanted in the superior maxillary bone. In the lower jaw there are on each side between 25 and 30 teeth. The basal ridges in the large teeth are sometimes transversely scored, and bifurcate as they approach the base, towards which the bifurcations gradually diminish in size; when the whole may not unaptly be compared to the contracted head of a small Pentacrinite. The anterior paddles are three times larger than the posterior ones, and, as compared with the other known species of *Ichythyosauri*, are relatively broader, and contain a greater number of digital ossicles*.

With respect to the size of this species, it appears to be second only to the *Ich. platyodon*.

In the museum of Viscount Cole there is a head of the *Ichthyosaurus communis* which measures in length two feet nine inches, indicating an animal of 20 feet in total length. The series of teeth in the dentary bone of the lower jaw is one foot ten inches and a half in extent.

The head of this species is expanded posteriorly, but quickly contracts to the base of the jaws, which are prolonged and somewhat compressed; towards their apices the profile abruptly converges to the tangential point. The basi-occipital bone in this species differs from that of the *Ich. platyodon* in having a shallower depression on the under part anterior to the condyle, and its length is greater in proportion to its breadth.

In the composition of the cranium may be noticed the very small size of the median frontals, and the great share which they have in the formation of the parietal foramen, of which only the posterior angle lies in the interparietal suture. The anterior and posterior frontals form exclusively the upper boundary of the orbit. There are seventeen sclerotic plates in each eye, the length of each plate being equal to half the diameter of the central circular space. The orbit and eye are relatively smaller than in the *Ich. tenuirostris* or *platyodon*. The nostril

* Mr. Hawkins first appreciated this character of the present species; he states that "the metacarpus has eight bones; the nine fingers contain no less than two hundred and twelve."—*Memoirs*, &c., p. 28. He changed the name assigned to the species by Mr. Conybeare, in order to express this peculiarity.

appears to be bounded by a straight line above and a curved one below. The upper maxillary bone receives the anterior part of the malar bone in a notch; and the slender, elongated malar forms the whole inferior boundary of the orbit.

The parietal transverse processes are excavated posteriorly for muscular insertions.

The extent of the symphysis of the lower jaw is nine inches, where the whole length of the jaw is two feet nine inches.

The surangular bone is perforated by a small foramen.

At the posterior part of the jaws the teeth diminish in size as they are situated further back. In the large head in Lord Cole's museum the longest tooth presents an exserted crown of two inches in length, and seven lines diameter at the base.

The surface of the cranial bones presents a silky appearance, owing to the fine striæ with which it is impressed.

The vertebræ are in slight degree more compressed in the antero-posterior direction than in the *Ich. intermedius*. I count forty vertebræ between the occiput and that which from its relative position to the iliac bones might be regarded as the sacral vertebra; from this to the extremity of the tail I have found 100 vertebræ; in all 140 vertebræ.

The humerus is shorter and stouter in proportion to its length than in any other species of the genus. The fore paddle is relatively broader than in the *Ich. intermedius*, and appears to have an additional series of digital bones. The component phalanges are transversely oblong; the larger ones are hexagonal or pentagonal, with the angles more or less rounded off; they become more rounded as they diminish in size towards the extremity of the digital series. I cannot perceive that distinction in the form of the phalanges of the *Ich. communis* as compared with those of the *Ich. intermedius* which led Mr. Hawkins to designate the latter *cheiro-paramekostinus* or oblong-boned. Neither does the pointed form of the paddles peculiarly characterize the *Ich. communis*.

After a careful comparison of the most perfect specimens of the anterior paddles of the two species, *Ich. communis* and *intermedius*, I am disposed to consider that the *Ich. communis* has an additional digital series, and about fifty additional phalangial ossicles, increasing upon the whole the breadth and power of the anterior fins. This increase in the strength of the powerful locomotive members accords with the more robust character of the head and teeth.

I am unable to fix upon any one useful distinguishing character in the hind paddle, between the *Ich. communis* and *intermedius*.

They resemble each other more closely in the structure of

their locomotive extremities than any other species of the genus. They are most easily and certainly distinguished from each other by the form and relative size of the head and teeth.

Localities.—The *Ich. communis* is the most common species in the lias at Lyme and Charmouth. It also occurs in the lias limestone at Street, but is much rarer there than the *Ich. intermedius*. Remains of the *Ich. communis* have been met with in the lias at Keynsham, and near Nembroth, Bristol. A beautiful head and other parts less complete of the *Ich. communis*, from Barrow-on-Soar, are preserved in the museum of the Philosophical Institution of Birmingham. This species is associated in the lias at Barrow with the *Ich. tenuirostris* and *intermedius*.

Prof. Sedgwick has parts of a specimen of this species from the lias of Stratford-on-Avon. Two skeletons of young *Ich. communis* in the Professor's collection both present the usual abrupt bend in the tail.

The *Ich. communis* is undoubtedly present in the lias at Boll in Wirtemberg, where it is associated principally with the *Ich. tenuirostris*.

Ichthyosaurus intermedius.

Mr. Conybeare thus characterizes this species: "The upper part of the teeth is much more acutely conical than in the *Ich. communis*, and the striæ less prominent, yet they are less slender than in *I. tenuirostris*;" and whereas in *I. communis* and *I. Platyodon* the coronoid (surangular) disappears on the outside, (being overlaid and concealed by the overhanging flap of the dental,) before the similar concealment of the angular bone, in *Ichthyosaurus intermedius* the angular draws itself up beneath the coronoid before the coronoid is thus covered up itself*.

The lower surface of the basi-occipital bone is but slightly excavated anterior to the condyle. The maxillary portion of the skull is relatively shorter, and converges more regularly to the snout, than in the *Ich. communis*; and the teeth are longer, more slender, and more numerous.

In a skeleton in Mr. Johnson's museum at Bristol I counted $\frac{40-40}{35-35}$ teeth.

The vertebræ present simple concavities at their anterior and posterior extremities; they increase in general size, and their spines grow wider in the antero-posterior diameter from the cervical to the pelvic region, and thence gradually diminish.

* Mr. Conybeare refers to the figure of a beautiful specimen (Pl. XVII. p. 112. vol. i., 2nd Series, Geol. Trans.) as displaying the latter structure. It is nevertheless obvious in that figure that the so called coronoid *x x* disappears beneath the dental *a* before the angular piece *v* similarly disappears.

The iliac bones are situated opposite the 44th and 45th vertebræ, both which have the two costal tubercles distinct. These become blended into a single oblong oblique surface at the 48th vertebra: this surface is situated as usual near the anterior and lower margin of the centrum. A space equal to one diameter and a half of the costal pit intervenes between them and the neurapophysial surfaces.

In a skeleton in Mr. Hawkins's collection there are 126 vertebræ. The two costal pits become blended at the 48th vertebra; the single tubercle disappears at the 76th vertebra, where the bend of the tail commences. Here three or four of the vertebræ are more compressed in the antero-posterior direction than those that immediately precede and follow them. The ribs are slender, and become flattened and longitudinally grooved at their distal extremities. They become straight, short, and like transverse processes after the 42nd pair.

There are 103 vertebræ in Mr. Johnson's fine skeleton of this species from Charmouth, but the series is not complete. In a beautiful skeleton of the *Ich. intermedius* from the lias of Lyme Regis, in the collection of Miss Conybeare, the tail exhibits the usual fracture or bend, which takes place at the 78th vertebra.

In the lower jaw the surangular is continued further forwards than the angular, before it is overlapped by the dentary; but this is not continued so far forwards as in the *Ich. communis.*

The surangular bone presents a longitudinal notch on its outside, which begins a little anterior to the articular cavity, and becomes gradually shallower as it advances forwards. The length of the lower jaw is to that of the vertebral column as 3 to 11.

In a small specimen of *Ich. intermedius* in Lord Cole's collection in which the upper maxillary bone measured 2 inches 8 lines, the diameter of the eye-plates was 2 inches, and that of the pupillary space 1 inch. I count 17 sclerotic plates.

The transverse portion of the episternum is rather longer and thicker than the median longitudinal piece.

The clavicles are very long and strong, especially at the middle; they are concave at their inner surface.

In the coracoid bone the humero-scapular articulation is of less extent than usual; the upper notch is as long and not deeper than the lower one; so that the neck of the scapulo-humeral articulation is longer than in the *Ich. platyodon* or *tenuirostris.*

The radial margin of the scapula is straight; the outer surface at the expanded humeral end is slightly excavated.

The fore-paddles have the same disproportionate size when compared with the hinder ones as in the *Ich. communis*; but they are not quite so broad in proportion to their length.

I have found seven series of digital bones in the more perfect specimens of the fore paddle. The first or radial digit divides after the fifth phalanx, and a supernumerary row of small bones is situated at the ulnar edge of the paddle. The distal ossicles present a similar transversely oblong hexagonal or pentagonal or rounded form as in the *Ich. communis*. I have seen in young specimens of both species a small perforation or pit in the centre of the phalangeal bones; this marks the situation of the entry of the blood-vessels. It is not, as Mr. Hawkins seems to suppose, a specific peculiarity of the *intermedius*.

This species appears to be the most common, if not the most generally distributed of the *Ichthyosauri*.

It is smaller than the *Ich. communis*. I have not seen any specimen exceeding seven feet in length.

Localities.—The remains of the *Ichthyosaurus intermedius* are common in the lias of Street; they have been found at Lyme Regis and Charmouth; in the lias near Weston, Bath, and Bristol; at Keynsham; in the lias at Charlton, about two miles from Cheltenham; and at Bedminster; in the limestone lias at Stratford, Warwickshire; likewise at Barrow-on-Soar, Leicestershire, but here less common than the *Ich. communis*. The *Ich. intermedius* also occurs in the lias near Whitby and Scarborough; in the lias of different parts of Yorkshire; at Bolsover in Nottinghamshire; at Whitton in Lincolnshire; at Walgrave in Northamptonshire. In almost every museum, indeed, I have found remains more or less perfect of this species.

The head of the *Ichthyosaurus* from the lias-field of Boll, figured by Prof. Jäger, does not belong, as he conjectures, to the *Ich. intermedius*, but to the species I shall presently have to describe under the name of *Ich. acutirostris*, and which is more nearly allied to *Ich. tenuirostris**.

Ichthyosaurus platyodon, Conybeare.
———— *giganteus*, Leach.
———— *Cheiroligostinus*, Hawkins.

The species which I am now about to describe is that which Mr. Hawkins has figured in his memoirs on *Ichthyosauri* (Pl. III.) under the name of *Cheiroligostinus*, and of which Dr. Leach had previously figured portions of the jaws and teeth under the name of *Ich. giganteus*. The skull and scapula

* The small specimen of *Ichthyosaurus* figured by Sir Everard Home under the name of '*Proteosaurus*' belongs to the present species and not to *Ich. tenuirostris* as Cuvier supposed.

figured in the *Philosophical Transactions* for 1814, Plates XVII.—XX., belong to the present species.

The name *platyodon*, proposed by Mr. Conybeare for the present species, is expressive of the form of the crown of the tooth, which is conical, subcompressed, the convex surfaces meeting on each side at a sharp edge: it further differs from the tooth of the *Ich. communis* in not having the basal grooves continued so deeply upon the crown, which, on the contrary, often present a smooth and polished surface*. The most prominent and distinctive character of this species is the equality of the fore and hind paddles as to size, and the comparative simplicity of their structure as to the number of digital phalanges and ossicles composing them. The discovery of this structure is due to Mr. Hawkins.

The head is relatively longer in proportion to the trunk than in the *Ichthyosaurus communis* or *Ich. intermedius*. The length of the trunk, *e. g.*, includes only one length and a half of the head, while in the *Ich. communis* it includes rather less than two lengths of the head, and in the *Ich. intermedius* rather more.

The head of the *Ich. platyodon* is also longer in proportion to its breadth than in the *Ich. intermedius*, but the jaws are relatively stronger on account of their greater relative breadth, and their less gradual attenuation to the rostral extremity. The lower jaw is remarkably massive and powerful, and projects further backwards beyond the joint than in the preceding species.

The orbit is relatively longer than in the preceding species: the upper maxillary bone is excluded from the formation of any part of its circumference by the union of the malar with the lachrymal bone. In a magnificent fragment of the cranium of this species in the collection of Mr. Johnson of Bristol the orbit measures one foot in diameter.

The nostril has a more elongated elliptical form than in the *Ichthyosaurus intermedius* or *Ich. communis*.

In the composition of the lower jaw I find that the angular bone is continued further forwards before it is covered by the overlapping dental piece than in the *Ich. intermedius* or *Ich. communis*, a structure which contributes to the strength of the lower jaw in this gigantic species.

The teeth I have not found to exceed 45-45 in the upper and 40-40 in the lower jaw. Their crowns are more frequently found to be snapped off than in the smaller species, a circumstance which is indicative of the greater violence with which they had been used.

* In the collection of Mr. Johnson of Bristol there is a tooth of the *Ich. platyodon* which measures two inches and a half in length: its crown, which, as usual, is pretty smooth and compressed, measures one inch.

The vertebræ have their bodies more compressed in the antero-posterior direction than in most other *Ichthyosauri*. From the occiput to the iliac bones I count 45 vertebræ, and thence to the end of the tail 75 vertebræ, making in all 120. From the dentata to the 25th vertebra inclusive, the centrum is characterized by two well-marked tubercles on each side for the articulation of the ribs. It is one of these vertebræ which is figured (in the inverted position with the spinal canal downwards) in Pl. XIV. of the *Philosophical Transactions* (1816), illustrative of Sir E. Home's second memoir on the *Ichthyosaurus*. The remaining vertebral centres present only a single surface for the articulation of the rib: the spinous processes are thicker, shorter, and more rounded superiorly, than in the *Ich. intermedius* or *Ich. communis*: their articular processes for mutual interlocking are well developed, especially at the anterior part of the spine.

The ribs commence, as usual, at the second cervical vertebra; they increase in length to the twenty-fifth, and thence diminish, at first gradually, but after the fortieth more suddenly. The forty-fourth pair is straight, and they are continued, gradually diminishing in length, attached by a simple head to the rudimental transverse process on each side of the body of the vertebra, as far as the 100th vertebra, counting from the atlas.

Pectoral Extremity.—The scapula* is a strong bone, with the upper or dorsal extremity truncated and slightly expanded, the anterior margin nearly straight, but slightly produced at its distal end; the posterior margin is slightly convex in the middle, moderately concave above, and very concave below; the inferior extremity is expanded to receive the articular ends of the coracoid bone and humerus; the posterior part of this articular extremity is the thickest part of the bone.

The coracoid has a more extended scapulo-humeral surface,—the scapular portion being the shortest, and has a narrower and deeper upper notch, than in the other species of *Ichthyosauri*. The internal surface of this bone is flat, but slightly concave below; the ento-sternal margin is thickened, the external surface is slightly convex. In Lord Cole's collection there is the coracoid of an *Ichthyosaurus platyodon* from Lyme, of which

* The following are the words in which two great comparative anatomists have recorded their opinions respecting the present bone when first presented to their consideration in a detached state. Sir E. Home, by whom it was first figured, says, (*Phil. Trans.*, 1818,) "It bears a greater resemblance to the first bone of the paddle than to any other; so that if the animal [meaning the *Ichthyosaurus*] has a posterior paddle this must belong to it." But the posterior paddle of the *Ichthyosaurus* being subsequently discovered, by which the assertion just quoted was disproved, Cuvier ventured an equally confident opinion respecting it, and says, "C'est un humérus de plésiosaurus, mais il ne resemble pas entièrement à ceux du squelette de Lyme." A lesson of caution in pronouncing an opinion on a detached fossil bone is strongly inculcated by the ill success which sometimes attends the guesses of even the best authorities.

the long diameter is 8 inches 4 lines, the short or transverse diameter 6 inches.

In the British Museum the scapulæ of an *Ich. platyodon* are preserved, each measuring 1 foot 5 inches in length, and 9 inches in breadth at the distal end.

The humerus is as short in proportion to its breadth as in the preceding species, but is more concave anteriorly than in the *Ich. communis* or *intermedius*. Its proximal rounded extremity is tuberculated at its circumference, and the shaft is grooved superiorly.

This character of the concavity or emargination of the anterior edge of the bone is present in a more marked degree in the radius or the anterior of the two bones which succeeds the humerus, and neatly distinguishes it from the corresponding bone of the *Ich. communis* or *intermedius.* There is no sufficient distinguishing character in the ulna, which differs only in size from that of the other *Ichthyosauri.*

The rest of the anterior paddle is composed of small transversely oblong bones, which gradually diminish in size to the extremities of the digits; and the limits of carpus and metacarpus can only be arbitrarily defined as in the other *Ichthyosauri.* The first or carpal row consists of three bones, as in the *Ich. intermedius,* but the radial or anterior ossicle is distinguished in the *Ich. platyodon* by an anterior emargination corresponding with that of the radius above. The same character is presented by the corresponding bone of the succeeding series, beyond which it is lost.

The whole paddle is not less clearly characterized, than the individual bones above mentioned, by the comparative paucity of its digital subdivisions; these do not exceed three in number, with two or three small supplementary ossicles on the radial margin of the paddle, which may be regarded as the rudiment of a fourth digit. The component phalangial ossicles are more rounded in their contour, and less transversely elongated, than in the previously described species: their margin is slightly raised on the outer surface. Beyond the antibrachium I count fourteen ossicles in each of the marginal, and fifteen in the median row, which, with the three supplementary ossicles, make forty-seven in the whole fore paddle or manus.

The pelvic bones are characterized by their relative superiority of size, more especially the ilium.

The femur, together with the other bones of the posterior member, is still longer in proportion than in the other species of *Ichthyosaurus.* It is only a very little less than the humerus: its proximal extremity presents a large depression, probably for the attachment of a stout ligament.

The tibia presents the same anterior emargination as the cor-

responding bone of the fore extremity, and the same character occurs in the two succeeding ossicles forming the anterior margin of the paddle.

The middle bone of the first or tarsal row is distinguished by a wedge-like process at its upper margin, which fits into the interspace of the tibia and fibula. There are three principal digital series as in the fore paddle, but the supplementary row contains a greater number of ossicles, and is situated on the posterior instead of the anterior side of the paddle.

The anterior digital series includes, counting from the tibia, nine ossicles, the median row eleven, and the posterior ten ossicles: there are eight small ossicles in the posterior rudimental digit.

Besides the comparative fewness of the digital ossicles in the paddle of the present gigantic species, they are characterized by their being placed at greater distances from each other in the terminal or lower half of the paddle, indicating that the ligamentous substance which connected them together entered more abundantly into the formation of the fin.

The imperfect spinal column, including 110 vertebræ, of an *Ich. platyodon* from Lyme, now in the British Museum, measures eighteen feet in length: but portions of the skeleton of the *Ichthyosaurus platyodon*, as the magnificent fragment of the cranium in possession of Mr. Johnson of Bristol for instance, have been discovered, which indicate individuals exceeding thirty feet in length.

Localities.—The lias of the valley of Lyme is the chief depository of this gigantic species, but its remains are pretty widely distributed: they have been found in the lias of Glastonbury, of Bristol, of Scarborough and Whitby, and of Bitton in Gloucestershire.

Vertebræ of this species occur in the lias at Ohmden, but not, apparently, at Boll, where the *Ich. communis* and *tenuirostris* occur: at least, the specimens referred, doubtfully, by Prof. Jäger to the *Ich. platyodon* have the characters of the *Ich. communis.*

Ichthyosaurus lonchiodon, O.*

A magnificent specimen of this species, measuring upwards of 15 feet, formed part of the second collection of Saurian remains purchased by Parliament of Mr. Hawkins, and now deposited in the British Museum.

The head somewhat exceeds, in relative size, that of the *Ich. platyodon*, to which the present species is closely allied: the jaws are deeper, and taper less gradually to their extremities. The teeth are more slender in proportion to their length than in the *Ich. communis* or *platyodon*, and are straighter than in the *tenuirostris* or *intermedius*. Their base is cylindrical, and re-

* λογχη, *hasta*, οδους, *dens.*

gularly fluted; a smooth boundary divides it from the crown, which is traversed by finer grooves converging to the apex; the transverse section of the crown is nearly circular, not compressed as in *Ich. platyodon*; it tapers gradually to the apex, which is nearer the posterior line than the central axis of the tooth.

The vertebræ are thicker in their antero-posterior diameter than in the *Ich. platyodon*: I count 45 between the occiput and pelvis, and 120 in the skeleton above-cited; but the tail is slightly imperfect. The scapula of the *Ich. lonchiodon* is more equably and deeply concave at the posterior margin, and its humeral extremity is relatively broader than in the *Ich. platyodon*. The bones of the extremities are thicker but shorter: the radius is emarginate anteriorly: there are three phalanges, of which the ossicles resemble in form those of the *Ich. platyodon*: but the whole paddle is relatively less. This difference is still more marked in the hind-paddle, which in the *Ich. platyodon* is, on the contrary, very nearly equal in size with the fore-paddle.

Locality.—The lias of Lyme Regis, where the skeleton above-noticed was discovered by Miss Anning.

*Ichthyosaurus tenuirostris**, Conybeare.
——— *grandipes*, Sharpe.
——— *chirostrongulostinus*, Hawkins.

The form of tooth figured by Mr. Conybeare as characteristic of the *Ich. tenuirostris* is one which it is very difficult to distinguish from that which is presented by the teeth of a species next to be described, and which in the form of its head is intermediate to the species called *Ich. intermedius* and *Ich. tenuirostris*. This species may, however, be recognised by other characters afforded by the humerus, the radius and tibia, and by the size and form of the paddle-bones, which have suggested the synonyms to their respective authors cited at the head of the present chapter.

But the most striking peculiarity of the *Ichthyosaurus tenuirostris* is that which Mr. Conybeare has happily chosen for its specific denomination, viz. the great length and slenderness of the jaw-bones, which are analogous in this respect to those of the Gharrial, and which, in combination with the large orbits and flattened cranium, give to its entire skull a close resemblance to that of a gigantic *Scolopax*, with a bill armed with teeth.

The length of the snout is chiefly due to the prolongation of

* The characters derived from the relative length of the head, trunk, and tail, and of the fore and hind paddle, quoted from Cuvier, and assigned to the present species by H. V. Meyer, in his work entitled "*Palæologica*," p. 214, are those of the *Ich. intermedius*, to which species the small specimen figured by Home, in the *Philosophical Transactions* for 1819, and now in the Museum of the Royal College of Surgeons in London, belongs, and not, as Cuvier supposed, to the *Ich. tenuirostris*.

the intermaxillaries, and their analogues the dentary pieces of the lower jaw. These latter pieces have a longitudinal groove on their external surface near the alveolar ridge. The surangular disappears beneath the dentary about half an inch anterior to the nostril; the angular continues longer visible on the outside of the jaw.

The teeth are more slender in proportion to their length than in any of the previously described species: I count from 65 to 70 on each side of the upper jaw; of these the posterior third, or about 25, are implanted in the slender maxillary bones. In the lower jaw there are about 60 teeth on each side. They are directed more obliquely backwards than in the species previously described.

The parietals are divided by a persistent sagittal suture, and the foramen is principally situated in this suture, the anterior part only encroaching between the frontals. Each of the posterior parietal bifurcations runs parallel with, and is applied to the outside of the supraoccipital bone. The median frontals are also separated by suture; they are relatively larger than in *communis*, but do not reach the margin of the orbit. The anterior frontals lie on the outside of the median frontals; the posterior frontals on the outside of the parietals. Cuvier states that the post-frontals form the whole of the posterior boundary of the orbit. In the *Ich. tenuirostris* this boundary is slender, and presents a fine and deep smooth groove next the orbit.

I have already alluded to the large size of the orbits: they are not less characterized by the slenderness of their inferior and posterior parietes; the diameter of the orbit equals that of the posterior or occipital region of the cranium.

The malar bone is singularly long and slender, and brings to mind its characteristic condition in Birds: its posterior extremity is joined with a slender curved descending process of the zygomatic bone. The rest of the zygomatic element is much more robust, and passes obliquely backwards to join the articular extremity of the tympanic bone, and to circumscribe the temporal fossa below. The temporal fossa is bounded by the posterior frontals, the parietal fork, and by a bone which Cuvier regards as peculiarly Ichthyosaurian, and which extends from the post-frontals to the end of the parietal fork. The nasal bones have the margin which forms the upper boundary of the nostrils, slightly convex, encroaching upon the nostril. The nostrils are narrow and elongated, but apparently larger in proportion than in the other *Ichthyosauri*, measuring two inches and a half long in a head two feet in length now in the British Museum, and figured by Mr. Hawkins in his 13th Plate. The rami of the lower jaw soon unite, and the symphysis extends through more than the anterior two-thirds of the jaw.

The vertebral column corresponds in its general slenderness with the characteristic form of the head. The number of constituent vertebræ appears to be at least as great as in any previously described species of *Ichthyosaurus*: they are more variable in the antero-posterior direction, and have a more rounded and less angular contour than in any other species: their anterior and posterior articular surfaces are simply concave. A greater proportion of the terminal caudal vertebræ present the laterally compressed form than in other *Ichthyosauri*, which would indicate that the tegumentary caudal fin was of greater relative extent. The atlas or odontoid epiphysis of the vertebra dentata separates more easily from the body of that vertebra than is usual in other species of *Ichthyosauri*; and there appear to be only two subvertebral bones, viz. that beneath the occipital condyle, which represents the atrophied body of the atlas; and the second, developed in the angle between the axis and odontoid epiphysis; the third, which is usually situated between the axis and third cervical vertebra, is wanting in the present species. This simplification of the apparatus for fixing the neck accords with the light and slender character of the head. The vertebræ gradually increase in thickness, or antero-posterior extent, as they approximate the caudal region, whence they gradually diminish in all their dimensions. But at the posterior part of the abdomen, and beginning of the tail, they are relatively thicker in the direction of the axis of the body than in the other species of *Ichthyosaurus*. I count fifty vertebræ between the atlas and the first caudal vertebra.

The costal tubercle in the caudal vertebræ is situated near the anterior part of the centrum. The ribs are long and slender, and appear, in the present species, gradually to increase in length to near the posterior end of the vertebral column, and then to shorten more abruptly than usual.

The extremities of the *Ich. tenuirostris* are characterized in the first place by a disparity in the size of the fore and hind pairs similar to that which obtains in the *Ich. communis* and *Ich. intermedius*, and the fore paddles are more particularly distinguished by their massive proportions, as compared with the vertebræ.

The clavicles are slender, slightly expanded at the middle, and contracted at the two extremities; their anterior margin is inflected about $1\frac{1}{2}$ inch from their sternal extremity. The scapulæ are relatively larger than in the preceding species, bent and expanded at their humeral extremity.

The coracoids have a broad neck, a slight inferior emargination, and a deep and narrow superior notch.

In a well-preserved specimen of *Ich. tenuirostris* in the Birmingham museum,

	In.	Lines.
The length of the coracoid was	4	5
The breadth	3	0
The length of the humerus of the same specimen	3	10
Its breadth at the distal end	3	0

The humerus is characterized by the superior length of its shaft, and the sudden hammer-like expansion of its distal articular extremity, which, however, presents the usual flattened shape. The anterior or sternal surface of the humerus is produced into a strong angular process; its dorsal surface is nearly flat. The radial condyle is most produced, and the breadth of the radius equals nearly the transverse diameter of two of the bodies of the parallel vertebræ of the spinal column. The ulna, and the rest of the bones of the extremity, bear the same large proportional size. In the *Ich. platyodon* the breadth of the radius hardly equals the transverse diameter of a single parallel vertebra. In the *Ich. communis* and *Ich. intermedius* the breadth of the radius is less by one third than the transverse diameter of a vertebra from the corresponding part of the body. There is a small notch between the radius and ulna, at their proximal end, in the *Ich. tenuirostris*.

It is the large size of the two antibrachial bones, and especially the radius, that renders necessary the hammer-like extension of the anterior condyle of the *humerus*. In a specimen of *Ich. tenuirostris* from the Grafton Quarry lias near Warwick, at present in the museum of the Philosophical Institution of Birmingham, the radius and ulna are anchylosed together, and to the humerus. The specimen described by Prof. Jäger seems to present the same condition. The radius is distinguished, like that of the *Ich. platyodon*, by an emargination at the middle of its anterior edge.

The manus commences by three transversely oval carpal bones, and includes only four digital series of ossicles, which present a more rounded figure than in the previously described species, but resemble most in this respect the paddle-bones of the *Ich. platyodon*. The radial bone of the first or carpal series is notched anteriorly like the radius itself, but this character is not marked in the next bone below, as in the *Ich. platyodon*: the radial finger is not bifurcated.

In the hinder extremity of the *Ich. tenuirostris* the femur, like the humerus, has a longer shaft than usual, and also a greater transverse extension of its inferior condyles. The tibia, like the radius, is notched at its anterior edge, but the emargination is relatively wider and shallower, and notched also in a slight degree in the corresponding ossicle of the tarsal series.

The ossicle which is wedged into the interspace between the tibia and fibula is relatively smaller than the corresponding bone in the other *Ichthyosauri*; it seems, therefore, rather to form a

part of the cnemial than of the tarsal series; this latter row of three bones being completed by the intercalation, in its middle, of an ossicle which forms part of the third or metatarsal row in the *intermedius* and *communis*. The character above noticed in the *tenuirostris* may be observed, in a slighter degree, in the *Ich. platyodon*.

Size.—In the museum of the Bristol Institution there is a magnificent though not complete skeleton of the *Ich. tenuirostris*, which measures thirteen feet in length. In Mr. Johnson's private collection, in the same city, there is a lower jaw of the *Ich. tenuirostris* from Lyme Regis, which measures two feet six inches in length: the exserted crown of one of the largest teeth in this specimen measures one inch and a half in length, and four lines in diameter across the base.

Localities.—The skeleton in the Bristol Museum is from the lias at Lyme Regis, where the species appears to be, however, less common than the *Ich. communis* and *platyodon*. The incomplete specimen from the lias of Stratford-on-Avon, in the museum of the Geological Society of London, and described by Mr. Sharpe under the name of *Ichthyosaurus grandipes*, belongs to the present species. Lord Cole possesses some characteristic fragments from the lias in the neighbourhood of Bristol. Evidences of the *Ich. tenuirostris* have been procured at Street and Walton, and at Barrow-on-Soar, in Leicestershire*.

This species undoubtedly exists in the lias formations of Boll and Amburg, in Wirtemberg†, and in the Jura limestone near Solothurn.

Ichthyosaurus acutirostris.

Under this name is indicated a species of *Ichthyosaurus*, which appears to be more common in the lias formations of the neighbourhood of Whitby than in those of Dorsetshire, although specimens also occur in the lias quarries of Street and Walton.

The teeth of the *Ich. acutirostris*, when they occur separately and singly, are hardly distinguishable from those of the

* Professor Sedgwick has an incomplete skeleton of the *Ich. tenuirostris* from this locality, in which the tail presents the abrupt and characteristic bend so common in the present genus; three of the ribs also of this interesting specimen have been fractured during the lifetime of the animal, and the fractured ends are rounded and expanded, and it is evident, that a false joint has been formed; the unintermitting respiratory movements having prevented a complete osseous reunion.

† The skeleton of the *Ich. tenuirostris* in the Gymnasium of Wirtemberg is in some respects more complete than are any of those yet preserved in the museums of this country, not excepting the beautiful specimen in the museum of the Philosophical Institution of Birmingham. It is well described and figured by Professor Jäger, in his treatise "De Ichthyosauri Fossilis Speciminibus," fol. 1824, and has enabled me to ascertain the number of ribs which surround the thoracic-abdominal cavity, and at the same time test the constancy of the character derived from the form of the humerus and the emargination of the anterior edge of the radius and tibia.

tenuirostris, although when a series of them are compared with a corresponding series of the *tenuirostris*, they are seen to be upon the whole a little wider at their base in proportion to their length. The most marked difference between these species is the length of the jaws; the intermaxillaries and dentary pieces being intermediate in this respect between the *Ich. intermedius* and *Ich. tenuirostris*.

The best example of the remains of the *Ich. acutirostris* which I have yet seen, is in the museum of the Natural History Society of Lancaster. It gives a profile view of the entire head, and of one anterior paddle.

The length of the head is eleven inches ten lines, and the alveolar dental series extends six inches six lines along the border of the jaws. The vertical diameter of the entire skull anterior to the orbit is three inches, and from this point both the upper and lower jaws regularly converge, in almost every direction to the end of the snout, which is sharper and more spear-shaped than in the other species.

The teeth vary in length from three to five lines, and about twenty-four may be counted in the space of three inches; they present a more regular alternation in length than I have observed in the other species of *Ichthyosauri*. There are about fifty teeth on each side of the upper, and forty on each side of the lower jaw, in all about 180; they are slightly bent backwards.

The orbit is relatively smaller than in the *Ich. tenuirostris*, but wider than in the *Ich. intermedius*; its inferior and posterior boundaries are thicker than in the *Ich. tenuirostris*.

I have not been able to found any characters on the structure of the vertebral column in the *Ich. acutirostris*. The humerus is relatively as long as in the *tenuirostris*, but is less expanded at the distal extremity. The radius presents the same anterior emargination as in the *tenuirostris* and *platyodon*. The phalangial ossicles are of an irregular rounded form, and are arranged in four digital series, presenting an arrangement as well as a relative size, which is intermediate between those which characterize respectively the *Ich. tenuirostris* and *intermedius*.

Mr. Hawkins has figured two snouts apparently belonging to this species in Pl. XIV. of his Memoirs, one of which, the larger and more complete specimen, was from the lias-quarry at Street, the other from that at Walton.

Besides the localities indicated in the preceding description, remains of the *Ichthyosaurus acutirostris* occur in the lias formation at Boll in Wirtemberg.

Ichthyosaurus latifrons, Kœnig. (Icones Sectiles, pl. xix.)

This species is founded on a specimen in the British Museum, including a portion of the cranium, of which the anatomy

is admirably worked out, and part of the vertebral column. Besides the character expressed in the specific name proposed by the accomplished Mineralogist and Palæontologist, whose name is associated with the genus, of which the present species forms so interesting an addition; the foramen parietale appears to be unusually large; and the articular surfaces of the bodies of the vertebræ present a flattened circumference.

Ichthyosaurus latimanus, O.

This species resembles the *Ichthyosaurus communis* in the ventricose, subobtuse character of the teeth, of which I have counted twenty-nine on one side of both jaws.

The articular surfaces of the vertebræ are only concave in the middle third part of their transverse diameter; the rest of the surface to the circumference is flat. They are stouter in the pelvic region than in the *Ich. communis*. The chief difference between this species and the *Ich. communis* obtains in the relative sizes of their anterior paddles. In an *Ich. latimanus* of six feet ten inches in length, and an *Ich. communis* five feet two inches in length, the following were the respective dimensions of the bones of the anterior paddle:

	Ich. latimanus.		*Ich. communis.*	
	Inch.	Lines.	Inch.	Lines.
Scapula, length of	3	4	3	0
———, breadth of humeral end .	1	8	1	3
Antibrachial bones, breadth of .	2	5	1	7
Length of entire paddle . . .	7	6	5	0
Breadth of ditto	3	6	2	8
Coracoid, intero-posterior diameter	3	8	2	4
———, transverse diameter . .	3	2	2	0

The clavicle was also proportionally powerful in the *Ich. grandipes*, and measured six inches eight lines in length.

The head is relatively shorter in the *Ich. latimanus* than in the *Ich. communis*; in the present specimen the lower jaw measures one foot four inches, while in the *Ich. communis* above cited the lower jaw measured one foot five inches.

In the nearly complete but dislocated skeleton in the museum of the Philosophical Institution at Bristol, on which the present species is founded, I counted 114 vertebræ; the terminal vertebræ of the tail presenting the compressed character indicative, as before noticed, of the former existence of a vertical tegumentary fin. Parts of the carbonized integument are preserved on the slab of lias on which this interesting fossil reposes; there is a broad patch about four inches beyond the last caudal vertebræ, being the first evidence I have yet met with of the actual presence of the caudal fin. The traces of tegument in the abdominal region are smoother than those figured in Dr. Buckland's Bridgewater Treatise.

If, as I have conceived, the pectoral fin and the massive sterno-coracoid arch relate to occasional reptation on the sea-shore, it may be inferred from the partial flattening of the articular surfaces of the vertebræ, in a species characterized by a greater size and strength of the fore paddles, that it was more terrestrial or littoral in its habits than the ordinary *Ichthyosauri.*

Ichthyosaurus thyreospondylus.

In the museum of the Bristol Institution there are five vertebræ of an *Ichthyosaurus*, of a compressed subpentagonal form; one of these, which has a vertical diameter of two inches and a half, and a transverse one of two inches and a quarter, is only nine lines in antero-posterior extent. The articular surfaces for the ribs are developed into short transverse processes, the upper one projecting immediately beneath the neurapophysial pit, and the lower one an inch lower down, and near the anterior margin; the anterior and posterior articular surfaces are concave, but have a convex rising, in the form of the heraldic fess, the base being equal to the breadth of the surface supporting the medulla spinalis, and the apex reaching to the centre of the articular surface. This character I have not observed in the vertebræ of any other species of *Ichthyosaurus*.

Ichthyosaurus trigonus.

I have been favoured by Miss Benett, of Norton House, Warminster, with some rare specimens from her valuable collection of fossil remains, among which is the body of a vertebra of an *Ichthyosaurus* remarkable for the straightness of the sides below the transverse process, from which point they converge at an angle of 70°; the upper part of the body of the vertebra, which supports the spinal and neurapophysial surfaces, is the broadest, and is bounded by a horizontal straight line, the whole presenting a triangular contour. The well-marked distinctions which this vertebra presents as compared with any that I have seen which belong to the preceding species, embolden me to regard it as indicative of a distinct species, for which a provisional name is proposed expressive of the form of the vertebra.

The anterior and posterior articular surfaces present the usual concavity: the non-articular surfaces at the sides of the vertebra are smooth.

	Inch.	Lines.
The antero-posterior diameter of this vertebra is	1	0
The transverse diameter	2	6
The vertical diameter	2	10

Locality.—Westbrooke, in Bromham, Wilts; Kimmeridge Clay.

Concluding observations.

In reviewing the principal facts which I have endeavoured to state succinctly in the foregoing pages, the first circumstance that may arrest the attention is the superior number of species which belong to the genus *Plesiosaurus*, as compared with the genus *Ichthyosaurus*; and since the circumstances which have led to the discovery and collection of the fossils of the two genera cannot be supposed to have materially differed, it may be concluded that the Plesiosaurs were rifer in the ancient seas of the secondary epoch, and manifested their typical structure under a greater variety of modifications than their more powerful and destructive congeners. To the four species of *Plesiosaurus* recognizably defined and described under the names of *dolichodeirus*, *macrocephalus*, *recentior*, and *triatarsostinus*, or *Hawkinsii*, I have been able to add descriptions or indications of twelve additional species; the number of *Plesiosauri*, of which remains have been discovered in the secondary strata of Great Britain, amounting now to sixteen.

To the species of *Ichthyosauri*, which Mr. Conybeare has so well defined under the names *communis*, *intermedius*, *platyodon*, and *tenuirostris*, a very great proportion of the fossils which I have examined are unquestionably referable, the small remainder affording evidence of only the six species indicated under the names of *lonchiodon*, *acutirostris*, *latifrons*, *latimanus*, *thyreospondylus*, and *trigonus*.

The deviations from the typical structure of the genus as exhibited in the common species called *Ich. intermedius*, which these additional evidences present, are of small amount, the chief being a greater proportional developement of the pectoral arch and its appended extremities, which characterizes the *Ich. latimanus*. In the other and better known species the principal modifications of the Ichthyosaurian type are manifested in the magnitude of the entire animal, the proportional developement of the head, the comparative length and slenderness of the snout, the proportional sizes of the fore and hind paddles, and the shape and number of the ossicles composing them. In the forms of the vertebræ there are but slight differences, and scarcely any in the proportions of the different regions of the vertebral column.

The part of the skeleton of the genus Plesiosaurus, which has been subject to the greatest extent of modification, is the cervical region, which becomes shorter and stronger as the head increases in size; but the general shape of the head appears to have presented less variety in the Plesiosaurs than in the Ichthyosaurs. The modifications of the vertebræ in the Plesiosaurs are many, though none are of very great extent. The differences of form which the bones of the pectoral and

pelvic arches present are greater than in the corresponding parts of the skeleton of the Ichthyosaurs, but those of the paddles themselves appear to be fewer; the number of carpal and tarsal bones varies from six to eight, but that of the digital ossicles is much more constant than in the Ichthyosaurs.

With respect to the geological relations of the Enaliosauria, or the extent of strata through which their relics have been traced, my researches are merely confirmatory of the generalizations already enunciated by Messrs. Conybeare and Buckland. The British Enaliosaurs extend through the whole of the oolitic period, including the lias and oolite proper to the wealden and chalk formations, the most recent depositary being the chalk marl, in which Ichthyosaurian remains have been discovered by Dr. Mantell, at Dover; Dr. Buckland has found similar remains in the Gault, near Benson, Oxon; and I have seen the humerus of a Plesiosaurus from the Gault, near Maidstone.

The following are the names of the species of *Enaliosauria*, in the order in which they are described in the foregoing Report:—

1. *Plesiosaurus Hawkinsii*, Owen.
2. ———— *dolichodeirus*, Conybeare.
3. ———— *macrocephalus*, Con.
4. ———— *brachycephalus*, O.
5. ———— *macromus*, O.
6. ———— *pachyomus*, O.
7. ———— *arcuatus*, O.
8. ———— *subtrigonus*, O.
9. ———— *trigonus*, Cuvier.
10. ———— *brachyspondylus*, O.
11. ———— *costatus*, O.
12. ———— *dædicomus*, O.
13. ———— *rugosus*, O.
14. ———— *grandis*, O.
15. ———— *trochanterius*, O.
16. ———— *affinis*, O.

1. *Ichthyosaurus communis*, Con.
2. ———— *intermedius*, Con.
3. ———— *platyodon*, Con.
4. ———— *lonchiodon*, O.
5. ———— *tenuirostris*, Con.
6. ———— *acutirostris*, O.
7. ———— *latifrons*, Kœnig.
8. ———— *latimanus*, O.
9. ———— *thyreospondylus*, O.
10. ———— *trigonus*, O.

Report on the distribution of Pulmoniferous Mollusca in the British Isles. By EDWARD FORBES, M.W.S., For. Sec. B.S.

THE object of this Report is to ascertain the geographical and geological distribution of Pulmoniferous Mollusca in the British Islands. I shall consider the subject under three heads:

1st. A view of the various influences which affect their distribution.

2nd. A detailed view of the distribution of the indigenous species in the various districts of these Isles.

3rd. The relations of this division of our native Fauna to the Fauna of Europe, and the distribution generally of the more remarkable species.

One hundred and one indigenous undoubted species of Pulmoniferous Mollusca inhabit the British Islands. Of these two belong to the genus Arion, five to Limax, one to Testacellus, one to Vitrina, two to Succinea, thirty-seven to Helix, three to Bulimus, two to Achatina, one to Azeca, five to Clausilia, one to Balea, twelve to Pupa, eleven to Planorbis, two to Physa, eight to Lymneus, two to Ancylus, one to Carychium, one to Acme, and five to Auricula.

These one hundred and two species are not equally distributed throughout the country, neither are they most numerous in the south, and decreasing gradually towards the north, or *vice versâ.* Their numbers vary in various places, and on inquiry we shall find this variation to depend on certain influencing causes, the two great primary influences being *climate* and *soil.* The influence of climate in Britain is indicated by the reduced number of species found in the more northern and colder districts, as compared with the number inhabiting the provinces of the south and centre. It is also indicated by the disappearance of species which inhabit all soils indifferently as we advance northwards, and by the presence of species in certain situations in southern and warm districts which usually avoid, or are sparingly found in such localities elsewhere. It is further shown by the tendency of individuals to multiply in temperate situations, and by the superior beauty of colouring displayed by species inhabiting warm districts.

Were climate the sole influence, the number of species would diminish as we advance northwards; but, though we find such a diminution very evident on comparing the extremities, the

increase of numbers in the central districts, as compared with some of the southern, indicates an influence in some cases more powerful than climate. This influence we shall find to be in its nature geological. It is the influence of the structure of a country on its existing Fauna.

The influence of the various kinds of rocks is very different and very important. Certain species, and even certain genera, appear to prefer certain rocks; but of all rocks limestone is the most favourable to the number and propagation of species. All kinds of lime rocks are not equally indifferent. Certain mollusks appear to prefer chalk, others oolite, others mountain limestone. Thus we find *Helix carthusianella* chiefly associated with chalk, *Helix pomatia* and *carthusiana* with the older tertiary, the cretaceous and oolitic formations, and *Helix scarburgensis* with the coal formation. Other species are distributed over all varieties of limestone rocks, but are not found on other kinds of soil, such as *Helix glabella*, *Helix lapicida*, and *Pupa secale*. The influence of all other rocks appears to be rather negative than positive; for, though several species limited in distribution seem confined to certain rocks in our country, we find them in other countries, where their distribution is more general, indifferent as to the soil in which they live. In general it is the mineralogical character of the rock which influences rather than its age. Limestone and sand influence all species as regards propagation, individuals multiplying to a much greater extent on calcareous and sandy soils than on slates, clay, or granite. Basalt has a similar influence, and primitive rocks generally are unfavourable either to the development of species or individuals. In certain cases the influence even of limestone may be completely neutralized by climate, as we find in Shetland, where the limestone tracts present no exceptions to the general paucity of species and individuals in those islands. Climate may also be seen overpowering the negative geological influence in Guernsey, where *Helix variabilis* multiplies to a great extent along with several other species on the unfavourable surfaces of granite and quartz rock. In some localities certain species are confined to certain rocks, which are generally distributed over all soils in others.

The order of influence of rocks on species is as follows, commencing with the most influential:

1. Cretaceous and oolitic.
2. Carboniferous rocks and trap.
3. Tertiary.
4. Saliferous.
5. Slates.
6. Granite and gneiss.

Though geographical and geological causes mainly regulate the distribution of Pulmoniferous Mollusca, there are circumstances which modify their influence in certain situations. One of the most important of these is *elevation*. In some countries, as in Switzerland, the influence of elevation is positive; that is to say, certain species occur at certain heights which are not found at a lower level. In Britain the influence of elevation is merely negative, the ascent of our mountains being characterized by the absence of species, the species becoming fewer as we ascend: towards the summit we find only *Helix alliaria*. The neighbourhood of mountains also affects a fauna, the upland species predominating around their bases even when the hills rise directly from the region of the plains. A wooded district is peculiarly favourable to the multiplication of land shells, especially the smaller Helices, Pupæ, and Clausiliæ, many of which we need not look for except in forests. The species of trees which grow in woods must also be regarded, pinewoods especially being much more unfavourable to a fauna than woods of any other kind. The presence of the aquatic Pulmonifera in a province must of course depend on the presence of water in the various forms of lake, river, ditch, and canal, each being characterized by its own peculiar species. The introduction of canals into a district must materially change the character of its aquatic fauna; and there are shells in the British lists, such as the *Dreissena polymorpha*, which owe their presence almost entirely to the construction of canals in their several localities. The nature of the beds of the lakes, streams and ditches in a district, affects the number and variety of species therein found. We need not look for many Limnei or Planorbes on gravelly bottoms, or for Ancylus on mud. The presence of many aquatic mollusca is determined by the plants growing in the localities they frequent. The neighbourhood of the sea exercises a most important influence on our fauna, both as regards the species and the multiplication of individuals. The marine influence would appear to be especially favourable to the propagation of species. The shells found on sea-banks are generally found in great numbers, as we see in the case of *Bulimus acutus*, *Helix ericetorum*, *Helix virgata*, *Pupa marginata*, and others. This is especially seen in the case of species common inland as well as near the sea, as *Vitrina pellucida*, and *Bulimus lubricus*. The presence of sand aids this multiplication of individuals, but is not indispensable. The marine influence appears to be favourable to the development of size and colour in a species. *Helix ericetorum* is generally found larger on the sea-side than

inland. The number of varieties of *Helix nemoralis* and *hortensis* found on sea-banks is much greater than when those species are gathered inland, and their colouring generally more vivid. When *Helix aspersa* is found plentifully near the sea, as in the Isle of Man, Ayrshire, and other places, the banding is much more distinct, and the ground colour brighter. Most species found near the sea vary very much, and many spurious species have been made in consequence, as we see in the cases of *Helix striata*, *Helix variabilis*, and *Bulimus acutus*. The last would appear to be confined in Britain to sea-banks, and to those of the western coast only. This does not apply to Ireland, where it is found both on the eastern and western coasts, and also inland. The difference between the faunas of the eastern and western coasts of England is very remarkable. Rivers influence the fauna of a district by the introduction of species not indigenous. Many of our local lists are swelled by the names of species collected from the *rejectamenta* of neighbouring streams. A stream flowing from a mountain-range into a plain will convey many species into the latter, which are properly inhabitants of the former. In Britain, where there is no *positive* influence of elevation, this is of comparatively little consequence; but the case is different on the continent, where large rivers, such as the Rhine and Danube, convey the inhabitants of the Alps into the plains of Germany. Even in our own country such rivers as the Thames and Severn are likely to give rise to many fallacies as regards the local distribution of a species. Man's agency may materially affect a fauna, and has affected that of Britain. *Bulimus decollatus*, *Bulimus clavulus*, *Clausilia solida*, and *Testacella Maugei*, have been introduced into the British lists by such means. In the cases of the above-mentioned species the carriage of plants from other countries has been the medium. It has been asserted that *Helix pomatia* was first introduced into Britain for purposes of food; but there are good grounds for regarding it as an aboriginal native. The thriving of such introduced species must depend on the localities to which they are transported; and should a species, the distribution of which is mainly influenced by geological causes, be introduced on a soil similar to that of its native habitat, it is especially likely to thrive and multiply. Species may be introduced into a local fauna by means of ballast. On the banks of the Frith of Forth we find numerous dead shells of *Paludina vivipara* and *impura*, and *Neritina fluviatilis*, none of which inhabit the district, but which have been conveyed thither in ballast, thrown into the water, and again cast up on the shore. Land

and fresh-water shells from the south of England may thus be found on the ballast-hills on the banks of the Tyne. Generally the shells are found dead; but we find *Helix carthusiana* living in Northumberland, having been introduced among ballast. Our marine fauna has been sadly vitiated by the same cause. Fallacies in judging of the distribution of shells may arise in consequence of the mixing-up of fossil with recent species. Where the fossils belong to the older strata, or even to the crag, such a mistake is not likely to occur; but where they belong to the Pleistocene period it is extremely difficult to distinguish. This more especially applies to marine shells: indeed, several undoubted Pleistocene fossils have found their way into the catalogues of living British mollusca. But it may also happen in the case of land and fresh-water species. In a Pleistocene bed at Portrush, in the north of Ireland, as has been noted by Mr. Smith, there are many species of land shells fossil, in such a state and in such a locality, that a person unaware of their history would, without hesitation, have enumerated them as natives of the place where they are found. It is remarkable, that among them there is not a single specimen of *Bulimus acutus* or *Helix ericetorum*, now abundant alive in the immediate neighbourhood of the bed.

All these modifying influences being taken into consideration, it behoves us to be very cautious how we judge of the influence of geological and climatal causes on the distribution of a species. The absence of a hill, a wood, a lake, or a ditch, may cause the absence of many species, and lead us to attribute their non-appearance in the district to a climatal cause, when the presence of the necessary modifying influence might have called them forth. It is only by a comparison of many districts, and of the face of the country in each, that we can hope to arrive at just conclusions; and it is necessary in every case to ascertain as far as we are able the circumstances under which each species is found in other parts of the world, especially in Europe, ere we can argue fairly on its distribution ni our own country.

II.—*On the Distribution of Pulmoniferous Mollusca in the various provinces of the British Isles.*

Dividing the British Isles into ten zoological provinces, we shall find that each presents certain features peculiar to itself as regards the Pulmonifera inhabiting it. These peculiarities arise from the predominance of some one of the influences which I have enumerated. It would be very desirable to consider the distribution in all the districts proposed by

Mr. Brand in his papers on the statistics of British Botany; but as yet the zoology of our country has not been sufficiently investigated, in many of our provinces, to warrant such a subdivision. In the adoption of the following districts I have rather followed zoological peculiarities than topographical limits.

	Districts.	Primary Influences.		Secondary Influences.	
		Positive.	Negative.	Positive.	Negative.
I.	The Channel Isles.	Climate.	Structure.	Marine.	Absence of Canals, &c.
II.	S.E. of England.	Climate. Structure.	....	Rivers, &c.	....
III.	S.W. of England.	Climate.	Structure.	Marine.	Want of water in parts.
IV.	N.E. of England.	Structure.	Climate.	Woods.	....
V.	N.W. of England.	Structure.	Climate.	Marine.	Elevation.
VI.	N. of Ireland.	Structure.	Climate.	Marine.	....
VII.	S. of Ireland.	Climate.	Structure.	Marine.	....
VIII.	S. of Scotland.	Structure.	Climate.	Woods.	....
IX.	N. of Scotland.	....	Climate. Structure.	Marine.	Elevation. Want of wood.
X.	Shetland Isles.	....	Climate.	....	Want of wood.

District I.—There needs some apology for including the Channel Isles in the preceding table. Botanists and conchologists have long been in the habit of enumerating their productions as members of the British Fauna and Flora; a better excuse, however, is, that by considering their inhabitants, in conjunction with those more truly British, we are enabled to connect, as it were, the natural history of our country with that of the continent, and thus avoid limited and local notions. The peculiarities of the first or more southern district are climatal; in this province we see an instance of positive climatal influence, and of the predominance of climate over geological structure. The islands are primitive, and, as far as rock goes, unfavourable to the development of Mollusca; nevertheless, shells rarely found in such situations, such as *Helix variabilis* and *striata*, are there seen in great numbers. The scarcity of ponds and lakes, and of water generally, accounts for the small list of fresh-water Pulmonifera, viz. three Limnei, one Planorbis, and one Ancylus. The Limnei are *L. pereger*, *L. palustris*, and *L. minutus*. In the island of Herm these are found in a situation deserving of notice. Here and there, among the low sandy banks formed by the sea, are springs of fresh water, forming little pools, the bottoms of which are sandy. In these pools we find the Limnei I have mentioned. The single Planorbis is *P. nitidus*. Among the Helices we find in this district two

species which do not occur elsewhere in the British Islands, and which may be considered indications of the positive influence of climate. They are *Helix naticoides* and *Helix revelata*. The total number of Pulmonifera, which I have observed in Guernsey and Herm, is twenty-nine species. I doubt not, on further investigation, half a dozen more may be added; but on the whole this district may be regarded as unfavourable to the multiplication of species in consequence of *structure*, yet favourable to the multiplication of individuals in consequence of *climate*. The fauna is nearly related to that of the opposite coasts of France, if anything, a little more southern in character.

District II.—In the second district, the south-east of England, the influences of climate and structure may be regarded as equally balanced. Here we only find *Helix obvoluta* and *Helix limbata*, which, as well as *Clausilia ventricosa*, may be regarded as climatal species. The only known localities for *Clausilia Rolphii* are in this district. Such land-shells as frequent chalky soils abound. In common with the south-west division, it furnishes *Helix pomatia*, *Testacellus haliotoideus*, and *Bulimus montanus*. The *Helix pomatia* is by many accounted an introduced species; but when we consider the partiality shown by that shell for the newer calcareous strata in all parts of Europe, and the geological correspondence of its British and continental habitats, I think there can be but little question of its indigenousness. The influence of a great river, such as the Thames, is more evident in the presence of the freshwater Pectinibranchia than of peculiar Pulmonifera. *Planorbis corneus* is chiefly found in this province. *Helix carthusianella* has not been found elsewhere in Britain. Some of the more northern species, such as *Helix scarburgensis* and *Clausilia dubia*, are absent. It is possible the absence of the rocks of the coal formation may cause the absence of several Helices and Pupæ in this district.

The Rev. Leonard Jenyns has favoured me with an excellent manuscript list of the land and fresh-water mollusca indigenous to Cambridgeshire, one of the most northern portions of this second province. The fauna of that county presents some peculiarities, in consequence of the presence of large tracts of fens, presenting features in common with the southernmost part of the fourth district. The following analysis of the localities of the Pulmonifera, enumerated in the catalogue of my distinguished correspondent, will prove instructive:—

The total number of undoubted species is sixty-two. Of these five are Limaces, one Vitrina, one Succinea, twenty-

three Helices, one Bulimus, two Achatinæ, two Clausiliæ, one Balea, five Pupæ, one Carychium, ten Planorbes, two Physæ, six Limnei, and two Ancyli. Of these, twenty-two species are generally distributed throughout the county; eighteen are local, found at a few places only, and those not fenny; seven are local species, having fenny localities, and nineteen are species found almost exclusively in the fen districts. Of the terrestrial species thirty-three frequent woods, gardens, hedges, and bushy places; four are found on heaths, dry banks, and open places; six in fenny districts, and one in cellars and damp buildings. Of the aquatic species seventeen are found in fens and low ponds, and four are not confined to the fens. Associated with these Pulmonifera, are one Cyclostoma, one Neritina (which is confined to the river Cam), two Valvatæ, three Paludinæ, one Anodon, one Unio, three species of Cyclas, and six species of Pisidium. In the list of Pulmonifera is also included with a query the *Physa alba*, of which Mr. Jenyns says he received a single specimen exactly according with Turton's figure, from a deep drain in the heart of the fens. "It was given me by a gentleman who was formerly resident in the neighbourhood of the spot, and who observed it with many others that had been thrown out with the mud on the occasion of the drain being cleaned. Not aware at the time of its being anything peculiar, only two or three specimens were brought away. I rather think the drain is now filled up." Such accidents well deserve our attention, frequently causing the destruction of perhaps the only locality in a district of some rare aquatic species. There are certain species, such as the Irish *Limneus involutus*, which are only known in one or two limited localities. An accident, such as that which destroyed this Physa, might render such local forms altogether extinct; and should it have been the fate of any such extirpated species to have remained unrecorded, in case it afterwards occurred fossil in a bed of fresh-water marl, a dangerous geological fallacy would take place. Mr. Jenyns notes another instance of the disappearance of an aquatic species. Of *Limneus glutinosus* he writes, "Some years back this species occurred in the utmost profusion in one marshy spot not a mile from my house; but it has since disappeared, and I never observed it in any other locality."

District III.—Throughout a great part of the third or south-western English district the negative influence of structure is very evident, most especially in that portion of it where climate should exhibit its influence most forcibly, namely, the counties of Cornwall and Devonshire. In these counties the

primitive structure of the rocks, doubtless, limits the number of species, but is overcome by the climatal and marine influences as respects the number of individuals. Several of the species too are decidedly southern and climatal in character, such as *Helix pisana* and *Testacellus haliotoideus*; others are western, as *Bulimus acutus*. The naturalization of some of the exotic Bulimi, such as *B. decollatus* and *B. clavulus* in this district, is a further proof of climatal influence. In the more eastern portions of the district, such as Dorsetshire, we find a general correspondence as regards species with those of the second district, doubtless dependent on a similarity of geological structure. Thus we find the western limits of *Helix lapicida*, *H. pomatia*, *Limneus auricularius*, and *Planorbis corneus*, on the southern coast, on the confines of Dorsetshire. The calcareous districts of Somersetshire and Gloucestershire are more favourable for the production of species, the catalogues of land and fresh-water mollusca in those districts presenting a considerable increase over those of Cornwall and Devon, dependent partly on geological causes, and partly on the greater frequency of localities for aquatic species. Throughout these counties the climatal influence is equally evident. The lists of the third province attain their maximum in South Wales, evidently dependent on the presence of carboniferous strata in that locality. There, however, climatal influence diminishes, and the *Testacellus haliotoideus*, so characteristic of that influence, disappears. In that portion of the district we find one of the two localities for the rare *Succinea oblonga*, which has been observed elsewhere in Britain only in the south of Scotland, in a district of similar geological structure. In the south-western province we find also one of the few British localities for another rare shell, the *Pupa cylindrica*, which was found by Mr. Jeffreys, who has done much towards the investigation of the mollusca of this part of Britain, in the neighbourhood of Bristol. The scarcity of aquatic species in the primitive counties of the third province is not attributed solely to the scarcity of water, but also to the nature of the sediment in pools and streams of primitive countries being evidently unfavourable to the multiplication of Mollusca.

District IV.—No part of Britain is richer in Pulmoniferous Mollusca than the north-eastern division of England. The Newcastle list alone, thanks to the researches of Mr. Alder, enumerates sixty-seven species. That of Scarborough, a district thoroughly investigated by Mr. Bean, exhibits no less than seventy-four. The former is a carboniferous neighbourhood,

the latter coralline oolite and chalk. We must attribute the richness of this district in species to the predominance of the positive geological influence. In the neighbourhood of Sunderland, in the magnesian limestone, sixty-four species occur. Of these the following are considered peculiar to the limestone in that locality: *Helix glabella, pulchella, rupestris,* and *variabilis, Clausilia dubia, Balea fragilis, Pupa pygmæa,* and *Acme lineata.* Judging from Mr. Bean's list, the calcareous strata of the neighbourhood of Scarborough must be especially favourable to the variation of species. Thus he mentions 152 varieties of *Helix hortensis,* fifty-eight of his *Helix pullata* (a white-mouthed form of *Helix hortensis*), 226 of *Helix nemoralis,* and twenty-one of its variety, *Helix notabilis.* Among the rarities of the north-eastern district, *Pupa alpestris, Pupa anglica, Helix scarburgensis* and *excavata,* and *Planorbis lævis* are conspicuous. In this district we find the northernmost British localities for *Helix carthusiana* and *lapicida, Acme lineata, Clausilia laminata, dubia,* and *ventricosa, Planorbis corneus,* and *Limneus glutinosus*; also for *Cyclostoma elegans* among the terrestrial Pectinibranchia. Dr. Greville has communicated the remarkable fact, that the *Helix aspersa,* so universal in England, is absent from the neighbourhood of Craven in this province.

In certain localities, Mollusca, generally distributed in most places without reference to geological structure, are confined to particular strata. This is remarkably shown in the following list of species observed by Mr. Bean to be peculiar to the calcareous strata in the neighbourhood of Scarborough: *Helix ericetorum, variabilis, umbilicata, caperata, radiata, pulchella,* and *lapicida; Bulimus obscurus, Achatina acicula, Azeca Matoni, Clausilia laminata* and *rugosa, Balea fragilis, Pupa umbilicata, marginata,* and *pusilla; Helix carthusiana* and *Helix umbilicata* confined to the chalk. In the neighbourhood of Bamborough *Helix caperata,* and *H. variabilis* are found together on trap, an unusual locality for the latter species.

District V.—The fifth or north-western division of England is similar in character to the fourth, though not so rich in species, those of the chalk and oolite being absent. The rarer species, found in the carboniferous strata of the last district, are found also in similar localities in this. The new red sandstone, constituting a large portion of this division, exercises no perceptible influence in favour of the increase of species. The presence of *Bulimus acutus* indicates the western climatal influence. *Planorbis corneus* is absent from the waters, and *Limneus stagnalis* appears for the last time on the western

coast of Britain. *Acme lineata* extends to Preston, the most northern limit, by the way, of *Paludina vivipara*. *Pupa alpestris* was first observed in Lancashire. In the slaty districts of North Wales and Cumberland the number of species is less than in other parts of the district. In the Isle of Man, which I append to this province, we find but a small list of species. *Planorbis marginatus* is altogether wanting in that island. The marine influence is evident there and in North Wales. *Helix ericetorum* is, in this province, a sea-side species. On the whole, this is the part of England least favourable to the multiplication of species, and where the influence of climate is either negative or neutralized by geological structure or marine influence.

Districts VI. and VII.—In Ireland we see the climatal influence predominating in the extreme south, but counteracted by geological structure, the geological influence predominating in the extreme north, but counteracted by the negative influence of climate; whilst the greater part of the intermediate districts presents the influence of carboniferous strata, modified by peculiarities of climate. The presence of Testacellus in the south is proof positive of climatal influence, as also is that of *Helix pisana* in the neighbourhood of Dublin. Generally speaking, however, the Pulmonifera of Ireland correspond to those of the extreme north of England and the south-west of Scotland. Ireland presents us with one aquatic species peculiar to it, the *Limneus involutus*, hitherto unobserved in any other part of the world, and by far the most remarkable of all the species of its genus. The number of true species of Pulmonifera found in Ireland is 75; thus exceeding Scotland by four, but falling 20 short of England. On the distribution of these, my friend Mr. Thompson, of Belfast, has favoured me with the following notes, as well as all the other information I possess on this division of the Irish Fauna. The difference in number between the species found on the north and south of Ireland is trivial as far as known, and cannot be stated with certainty. Of species found in the more southern half of Ireland, and not in the north, the following may be mentioned: *Testacellus haliotoideus* (var. *scutulum*), at Youghal; *Helix globularis, Helix striata, Achatina acicula,* and *Limneus involutus*. *Clausilia bidens*, which has been obtained in Cavan and Fermanagh, and *Planorbis lævis*, common to Down and Antrim, have not been observed in the south. *Helix pisana* is only found in the county of Meath. *Planorbis corneus* is found only towards the centre (as to latitude) of Ireland. The chalk district of the north seems to have little influence on the species of the Pul-

monifera, although it has much in increasing the number of the species indigenous to it. The difference between the catalogues of *Mollusca pulmonifera* of Cork, Dublin and Belfast, is not great either as to number or otherwise. *Helix glabella*, common about the two former, is not obtained near Belfast. *Helix pomatia, carthusiana* and *lapicida*, and *Limneus elongatus*, appear to have found their way into the Irish catalogues by mistake. In Ireland the distinction between the Faunas of the eastern and western coasts is not so marked, as respects the *Pulmoniferous Mollusca*, as in England.

District VIII.—The southern half of Scotland might be divided into two great geological districts, the one favourable and the other unfavourable to the development of its Fauna. The unfavourable portion is the southernmost, and consists chiefly of slaty rocks. The other division, extending to the edge of the Grampians, is mainly composed of rocks of the coal formation, and of trap, both favourable to the production of mollusca. But climate here almost neutralizes the geological influence; the effect of which may, however, be still recognised in the multiplication of individuals on a genial soil. For the last time in Britain we meet with *Succinea oblonga, Helix fusca, globularis, pura, aculeata, pygmæa* and *striata, Bulimus obscurus, Achatina acicula* (these exceeding rare), *Pupa anglica, substriata, pygmæa, edentula* and *pusilla, Azeca tridens,* all the species of Planorbis and of Physa, and all the Limnei except *Limneus pereger* and *Limneus minutus,* as also *Ancylus lacustris.* The chief rarities of the south of Scotland are *Succinea oblonga* and *Pupa cylindrica*; the former found on carboniferous sandstone, the latter on trap, and neither of them as yet observed in the north of England. In the woods of the district we find *Helix scarburgensis* and *fusca, Pupa edentula*, and occasionally *Bulimus obscurus.* The portions most prolific in species are the neighbourhoods of Edinburgh, Berwick, and Glasgow, partly, without doubt, in consequence of the woods near those towns. There is but little difference between the Pulmonifera of the eastern and western divisions of the district; but among the fresh-water Pectinibranchia we find *Paludina impura* on the western coast only. The south of Scotland may be regarded as upland.

District IX.—Climate sways the distribution in the ninth district. The hospitality of the Highlands does not extend to snails. The bleak granite mountains, with their scant vegetation, hold out but few temptations to the Pulmonifera. The species are few, and the specimens are few. On the trap and under the marine influence, in some of the Western Isles, they are some-

what more numerous than elsewhere. Some rare species, however, occur, with several of the scarcer forms; *Helix fulva*, *scarburgensis* and *excavata*, have lately been added to the list by Mr. Alder. *Helix crystallina* is not uncommon; *Helix ericetorum* and *Bulimus acutus* abound on sandy soil in the outer Hebrides. *Pupa palustris* and *Helix radiatula* were found on the islands opposite Oban by Mr. Jeffreys, and *Pupa cylindrica* in Skye (on trap) by Mr. Macaskill. On the mountains *Helix alliaria* is not rare.

District X.—Only five species of *Pulmoniferous Mollusca* inhabit the Shetland Isles. These five are *Arion ater*, *Limax cinereus*, *Vitrina pellucida*, *Helix alliaria* and *Limneus pereger*, all species common to the whole of the north of Europe, and extending their range to Greenland. The geological structure of these islands being primitive is unfavourable to the development of species; but I regard the distribution as wholly climatal in this, the most northern province, inasmuch as it is in no way influenced by the tracts of limestone which occur in certain localities in Shetland. Individuals are as scarce as species; the only animal of those enumerated at all plentiful is *Arion ater*.

In the first of the two following tables the numbers of the species of each genus found in the various districts are exhibited. For the first province the materials were derived from personal research; for the second, from published lists and communications from Mr. Jenyns and Dr. Stanger; for the third and fifth, from published lists and personal observation; for the fourth, from the published lists of Mr. Alder, and communications from that gentleman, Mr. Bean and Dr. Greville. The sixth and seventh districts yielded their numbers through the medium of my friend Mr. Thompson, of Belfast; the eighth and ninth from personal observation, published lists, and communications from Sir William Jardine, Dr. Johnston, and Mr. Smith of Jordanhill, and other gentlemen. The tenth I investigated myself.

In the second table, the distribution of the species on the various strata is exhibited. If the *pectinibranchous Mollusca* had been added, the preponderance of the cretaceous and oolitic strata would have been much more evident, and there are several species at present known entirely as inhabitants of the carboniferous rocks, which, I doubt not, will also be found on the former, or have been confounded with other species.

In both tables I have omitted all forms which I could not regard as true species, or the indigenousness of which has been questioned on good grounds.

Table I.

GENERA.	I.	II.	III.	IV.	V.	VI. & VII.	VIII.	IX.	X.
Limax	3	6	4	6	3	6	6	2	2
Testacellus...	1	1	1	0	0	1	0	0	0
Vitrina	1	1	1	1	1	1	1	1	1
Helix	14	32	29	33	24	28	24	16	1
Succinea	1	1	2	1	1	1	2	1	0
Bulimus......	1	2	3	1	2	2	1	1	0
Achatina......	1	2	2	2	2	2	2	1	0
Acme	—	1	1	1	1	1	0	0	0
Azeca	—	1	1	1	1	0	1	0	0
Pupa	2	9	10	9	8	9	9	3	0
Clausilia.. ...	1	4	3	4	2	2	1	1	0
Balea	1	1	1	1	1	1	1	0	0
Carychium...	—	1	1	1	1	1	1	1	0
Limneus	2	7	4	7	6	6	6	2	1
Planorbis ...	1	10	9	10	8	10	10	0	0
Physa	—	2	2	2	2	2	2	0	0
Ancylus	1	2	2	2	2	2	2	1	0
TOTAL ...	30	83	76	82	65	75	69	30	5

Table II.

Rocks in order of their Influence.	Limax.	Testacellus.	Vitrina.	Helix.	Succinea.	Bulimus.	Achatina.	Acme.	Azeca.	Pupa.	Clausilia.	Balea.	Carychium.	Limneus.	Planorbis.	Physa.	Ancylus.	TOTAL.
Cretaceous and Oolitic	6	1	1	29	1	3	2	1	1	9	4	1	1	7	10	2	2	81
Carboniferous Rocks and Trap	6	1	1	28	2	2	2	1	1	12	3	1	1	6	9	2	2	80
Tertiary	5	1	1	26	1	2	2	1	0	7	3	1	1	7	10	2	2	72
Saliferous	4	0	1	25	1	1	2	1	1	8	3	1	1	6	7	2	2	65
Slates	4	1	1	18	1	2	1	1	0	2	1	1	1	3	5	1	1	44
Granite & Gneiss	3	1	1	18	1	1	1	0	0	2	1	1	1	2	1	0	1	35

III.—*On the relations of the British Pulmoniferous Mollusca to those of Europe generally, and the distribution of the more remarkable species.*

To compare the distribution of Pulmonifera in Britain with their distribution on the Continent, or even to ascertain the European range of British species, is by no means an easy task. The difficulty chiefly arises from the want of agreement between the writers of different countries. Almost every land and fresh-water shell has half a dozen synonyms, and every local catalogue presents us with names (not species) peculiar to itself. The habit of changing names,—I had almost said,—wantonly,—has been indulged in to an unwarrantable extent among Malacologists, especially writers on the order of ani-

mals under review. Unfortunately a great part of the European species were named contemporaneously by Montagu in England, and by Draparnaud in France, and several years elapsed before it was possible to compare their nomenclatures. In the mean time that of the former had become universal among British authors, and that of the latter among Continental, and much confusion has arisen in consequence. Of late there has been a tendency among British authors to adopt Draparnaud's names, and among Continental writers to use those of Montagu. Another cause of difficulty has arisen from the very different views entertained by authors as to the specific claims of the forms they describe, some elevating every little variation to the rank of a species, others attempting definitions more in accordance with the philosophy of natural history. Writers acquainted only with the animals of their own country or district, are especially apt to blunder on this point, and to constitute every local variety a species. The error is luckily on the safe side: it is better that every variation should be dignified as a species for a time, than that any one form be past over. In the following table, exhibiting a comparison of the principal published European lists, allowances must be made for the above reasons, though to a certain extent I have endeavoured to correct it. I may mention that Krynicki's Russian list includes the Caucasus. The French list may be regarded as slightly over-stated, several of Michaud's species being probably supposititious. Deduct one Limneus from the British list, and there remain the numbers for England alone.

	Limax.	Parmacellus.	Testacellus.	Vitrina.	Helicophanta.	Helix.	Succinea.	Bulimus.	Achatina.	Acme.	Azeca.	Pupa.	Clausilia.	Balea.	Carychium.	Limneus.	Planorbis.	Physa.	Ancylus.	TOTAL.
British Isles	7	0	1	1	0	36	2	3	2	1	1	12	5	1	1	8	11	2	2	96
Scotland	7	0	0	1	0	24	2	2	2	0	1	9	1	1	1	6	10	2	2	71
France (Michaud) ...	12	0	1	4	0	78	2	7	3	1	1	25	11	1	1	10	13	4	2	176
Sweden (Wilson)	9	0	0	1	0	20	1	1	2	0	0	6	4	0	1	10	9	2	2	69
Brabant (Kickx)	8	0	0	2	0	22	1	4	2	0	0	12	6	1	1	10	11	2	2	84
Switzerland (Charpentier)	7	0	0	4	0	40	2	3	2	1	0	21	11	1	1	6	12	2	2	115
Italy, exclusive of Sicily (Jan)	2	0	0	2	1	66	2	2	4	1	0	13	10	0	1	6	5	2	2	119
The two Sicilies (Philippi)	?	3	0	1	0	43	2	5	3	0	0	5	7	0	0	5	2	2	1	79
Germany (Pfeiffer) ...	6	0	0	3	2	58	2	4	3	1	1	15	17	1	1	10	11	2	2	139
Russia, with the Caucasus (Krynicki)	?	0	0	1	0	47	2	6	3	0	0	16	15		1	12	13	2	1	119

The Limacidæ of the Continent cannot well be compared with those of Britain, as they have been but imperfectly observed in most countries, and in many Faunas omitted altogether. France and Sweden appear to excel us in the number of species; but it is probable we possess several unnoticed forms. Of the two species of Testacellus generally enumerated as British, one, the *T. Maugei,* found near Bristol, was undoubtedly introduced; it is a native of the Canaries. The other is a true native, found generally throughout the south-western countries of Europe, the distribution of which may be regarded as regulated by climate. Our single species of Vitrina is a shell found in most parts of Europe, in all the northern districts especially. It occurs also in Greenland. Its distribution appears to be essentially climatal, as is the case with the genus generally, of which the European forms are the northern representatives, the true centre of the genus being near the tropics. The distribution of the genus Succinea appears to be similarly regulated; but our common British species, the *Succinea amphibia* (including *S. gracilis* or *Pfeifferi*), is much more widely spread than any Vitrina, being found throughout Europe, from Archangel downwards, in North America, and in North and South Africa, as far as the Cape of Good Hope. The *Succinea oblonga* has also a very wide range. In Europe it occurs in Britain, Denmark, France, Germany, and Switzerland, and is found at Lima, Vera Cruz, and the Cape of Good Hope.

In the number of native species of the genus Helix, England exceeds Scandinavia by 17 species, and Brabant by 15, but yields to the other European lists of equal importance, especially to those of the southern countries. France exceeds Britain by no less than 41 species. The only species which can be looked upon as certainly peculiar to the British Isles is the *Helix fusca* of Montagu; the *Helix scarburgensis,* till lately considered as exclusively British, having been found in Northern Germany. It is difficult to say whether any of the Helices allied to *H. nitens* are confined to Britain, so much confusion prevailing in the Continental lists as regards that tribe. Thus in no foreign catalogue do we find *Helix alliaria,* though without doubt it occurs in most parts of Europe. I have collected specimens in France and elsewhere. It extends to Greenland. This interesting subdivision of the genus prevails most in the moor-land or elevated districts of England. We find such to be the case also abroad. *Helix cellaria* is found throughout Europe, and the countries bordering on the Mediterranean. *Helix nitida* extends its range

to the United States and to the West Indies. The allies of *Helix variabilis* are extensively distributed. *Helix variabilis* itself is found through most parts of Europe, the Mediterranean countries, and the United States of America; and *Helix pisana* has an equally extensive range. The *hispid Helices* are generally distributed through Europe. Much confusion, however, exists in lists as regards the immediate allies of *Helix hispida*. The Helicogenæ of Ferussac have great ranges. The common snail of our gardens, *Helix aspersa*, is equally common in the gardens of Southern Europe, and is found also in parts of Asia, Africa, and North and South America. *Helix hortensis*, *nemoralis* and *arbustorum*, inhabit most parts of Europe; and *Helix pomatia* has nearly as great a range as *Helix aspersa*.

Bulimus montanus, with us an inhabitant of the plain districts of southern England, on the Continent inhabits subalpine districts, chiefly where limestone prevails. Our *Bulimus obscurus* is found throughout Europe. *Bulimus acutus*, with us usually an inhabitant of the sea-side, is found inland in Switzerland. On the shores of the Mediterranean it abounds, and assumes many variations of form and colour, along with its near ally *Helix conoideus*, near which it should probably be placed, rather than with the Bulimi. Both our Achatinæ are natives of most parts of Europe, and the *A. acicula* is found in Northern Africa. As we approach the tropics the forms of this genus multiply. Our species belong to the section Cionella (Jeffreys), the centre of which may be regarded as placed in the islands of the north-west coast of Africa. *Azeca Matoni* is a native of Central Europe as well as of Britain. The number of British species of Clausilia is but a small proportion of this large and interesting genus, the varied forms of which abound in the countries of Eastern Europe and the neighbouring parts of Asia. One of our native forms, however, the *Clausilia Rolphii* of Leach, is confined to Britain, and with us is only found in the south-east of England. Our other species are found in most of the countries of the Continent. The *Balea fragilis* is frequent in Northern and Central Europe, and is the only European species. Other forms of the genus occur in the West Indian Islands. The genus Pupa affects mountainous districts, and species abound in the Alps, where we find many eccentric and abnormal forms of the genus. France, Russia, and Austria exceed us in the number of species, France doubling our number. One of our Pupæ, the *Pupa anglica*, has never been found out of the British Islands; the others are generally distributed through Europe, and the *Pupa umbilicata* inhabits Mount Atlas.

The distribution of the Aquatic Pulmonifera is much more equal than that of the Terrestrial, as might be expected from the nature of the element in which they live. In Limneus, Planorbis, Physa, and Ancylus, the differences between the European lists are very slight, and the species are generally identical. The *Limneus involutus* of Harvey would appear to be the only species peculiar to Britain, not having been found elsewhere than in its Irish locality. Some of our native species have very wide ranges, *Limneus stagnalis* extending from Cachemyr to the United States, and *Limneus palustris* occurring in most parts of the world. *Limneus pereger* is also very widely distributed. *Limneus minutus* is found in very elevated situations in the Alps, and in low ground in the north of Africa. The genera Limneus, Physa, and Planorbis, are found to inhabit most parts of the world.

Table

Of the Species of Pulmoniferous Mollusca inhabiting the British Isles, and their Geographical Distribution.

	Genera and Species.	British Distribution.	General Distribution.
I.	Limax, Linn.		
	[Arion.]		
1	Empiricorum, Fer.	I.—X.	All Europe, Teneriffe.
2	hortensis, Fer.	II.—VIII.	Central & Northern Europe.
	[Limax.]		
3	cinereus, Linn.	I.—X.	Europe, Teneriffe, Algiers.
4	variegatus, Fer.	II.	Europe, Cyprus, U. States.
5	agrestis, Linn.	I.—VIII.	All Europe, Teneriffe.
6	Sowerbii, Fer.	II., VII.	France.
7	brunneus, Drap.	VIII.	France.
II.	Testacellus, Cuv.		
1	haliotoideus, Drap.	I.—III., VII.	S. W. of Europe.
1½	scutulum, Sow.	II., VII.	
III.	Vitrina, Drap.		
1	pellucida, Müll.	I.—X.	N. & C. Europe, Greenland.
1½	*Drapernaldi*, Jeff.		
1⅓	*Diaphana*, Jeff.		
1¼	*Dilwynii*, Jeff.		
IV.	Succinea, Drap.		
1	amphibia, Drap.	I.—IX.	All Europe, N. America, Africa, Western Asia.
1½	*gracilis*, Alder. (Pfeifferi), Rossm.		
2	oblonga, Drap.	III., VIII.	N. & C. Europe, S. America, Cape of Good Hope.
V.	Helix, Linn.		
1	pomatia, Linn.	II., III., ?	C. & S. Europe, W. Asia, N. Africa, S. America.
2	arbustorum, Linn.	II.—IX.	N. E. and C. Europe.
3	aspersa, Mull.	I.—IX.	C. & S. Europe, Asia, Africa, N. & S. America.

	Genera and Species.	British Distribution.	General Distribution.
4	naticoides, Drap.	I.	S. Europe, N. Africa.
5	nemoralis, Linn.	I.—IX.	All Europe.
5½	*hybrida*,		
6	hortensis, Linn.	I.—IX.	All Europe.
7	limbata, Drap.	II.	Central Europe, Rhodes.
8	carthusiana, Drap.	II.—IV.	C. & S. Europe, N. Africa, Caucasus.
9	carthusianella, Drap.	II.	C. S.& E. Europe, W. Asia.
10	obvoluta, Mull.	II.	C. Europe, Sweden.
11	glabella, Drap.	II.—V., VII.	C. Europe.
12	depilata, Pfeiff.	I.—VIII.	All Europe.
12½	*concinna*, Jeff.		
13	hispida, Mull.	I.—IX.	N. and C. Europe, Tauria.
13½	*sericea*, Mull.		
14	globularis, Jeff.	I.—VIII.	N. and C. Europe.
15	revelata, Fer.	I.	France.
16	fusca, Mont.	II.—VIII.	?
17	excavata, Alder.	IV., V., VII., VIII.	
18	lucida, Drap.	II.—VIII.	N. C.& E. Europe, Algiers.
19	nitidula, Drap.	I.—IX.	All Europe.
19½	*Helmii*, Gilb.		
19⅓	*glabra*, Studer.		
20	radiatula, Alder.	II.—IX.	France.
21	alliaria, Miller.	I.—X.	N.& C. Europe, Greenland.
22	cellaria, Mull.	I.—IX.	All Europe, W. Asia, N. Africa, Canaries.
23	pura, Alder.	II., IV.—VIII.	Central Europe.
23½	*nitidosa*, Fer.		
24	crystallina, Mull.	II.—IX.	All Europe.
25	fulva, Drap.	II.—IX.	N. and C. Europe.
25½	*Mortoni*, Jeff.		
26	scarburgensis, Bean.	IV.—IX.	North of Germany.
27	aculeata, Mull.	II.—VIII.	N. and C. Europe.
28	pulchella, Mull.	II.—IX.	N. & C. Europe, U. States.
29	pygmæa, Drap.	II.—IV., VI.—VIII.	N. and C. Europe.
30	rupestris, Drap.	III.—VIII.	N. and C. Europe.
31	rotundata, Mull.	I.—IX.	All Europe.
32	striata, Drap.	I.—VIII.	All Europe, W. Asia.
33	variabilis, Drap.	I.—VII.	C. & S. Europe, N. Africa, W. Asia, N. America.
34	ericetorum, Linn.	I.—IX.	All Europe, W. Asia, Egypt.
35	pisana, Mull.	III., VII.	S. Europe, W. Asia, N. Africa, N. America, United States, Canaries.
36	lapicida, Linn.	II.—IV.	All Europe.
VI.	BULIMUS, Brug.		
1	acutus, Mull.	I., III., V.—VII., IX.	C. & S. Europe, N. Africa, W. Asia, N. America.
2	montanus, Drap.	II.—III.	C. and E. Europe.
3	obscurus, Mull.	II.—VIII.	N. C. & E. Europe, Caucasus.
VII.	ACHATINA, Lam.		
1	acicula, Mull.	II.—IV., VII., VIII.	All Europe, N. Africa, Caucasus.
2	lubrica, Mull.	I.—IX.	All Europe.
VII.	AZECA, Leach.		
1	Goodalli, Fer.	II.—V., VIII.	Germany, France, Pyrenees.

	Genera and Species.	British Distribution.	General Distribution.
VIII.	CLAUSILIA, Drap.		
1	bidens, Mull.	II.—VII.	All Europe.
2	ventricosa, Drap.	II.—IV.	Central Europe.
3	Rolphii, Leach.	II.	?
4	dubia, Drap.	IV.	Central Europe.
5	rugosa, Drap.	I.—IX.	All Europe.
5½	*Everettii*, Miller.		
IX.	BALEA, Gray.		
1	fragilis, Drap.	I.—VIII.	N. C. and E. Europe.
X.	PUPA, Drap.		
1	umbilicata, Drap.	I.—IX.	All Europe, Caucasus, N. Africa.
2	marginata, Drap.	I.—VIII.	Europe, Caucasus.
3	anglica, Fer.	IV., VI., VIII.	?
4	secale, Drap.	II., III., V.	All Europe.
	[VERTIGO.]		
5	edentula, Drap.	II.—VIII.	N. and C. Europe.
6	cylindrica, Fer.	III., VIII., IX.	Central Europe.
7	pygmæa, Drap.	II.—VIII.	N. and C. Europe.
8	alpestris, Fer.	IV., V.	Central Europe.
9	substriata, Jeff.	II.—VIII.	Central Europe.
10	palustris, Leach.	II.—IX.	Central Europe.
11	pusilla, Mull.	II.—VIII.	Europe.
12	angustior, Jeff.	II., III., VII.	France.
XI.	CARYCHIUM, Mull.		
1	minimum, Mull.	II.—IX.	N. E. and C. Europe.
XII.	ACME, Hartmann.		
1	lineata, Drap.	II.—VII.	Central Europe.
XIII.	PLANORBIS, Mull.		
1	corneus, Linn.	II.—IV., VII.	Most parts of Europe.
2	marginatus, Drap.	II.—VIII.	Most parts of Europe, Algiers, Caucasus.
2½	*rhombeus*, Turt.		
2⅓	*turgidus*, Jeff.		
3	carinatus, Mull.	II.—VIII.	Europe generally, Caucasus
3½	*disciformis*, Jeff.		
4	vortex, Mull.	II.—VIII.	N. & C. Europe, Volhynia.
5	spirorbis, Mull.	II.—VIII.	Most parts of Europe.
6	lævis, Ald.	IV., VI.	France.
7	albus, Mull.	II.—VIII.	N. and C. Europe.
7½	*deformis*, Lam.		
7⅓	*glaber*, Jeff.		
8	contortus, Linn.	II.—VIII.	N. E. and C. Europe.
9	lineatus, Walker.	II., VI., VII.	Europe, Caucasus.
10	nitidus, Mull.	I.—VIII.	Europe.
11	imbricatus, Mull.	II.—VIII.	Europe.
XIV.	PHYSA, Drap.		
1	fontinalis, Linn.	II.—VIII.	N. and Central Europe.
2	hypnorum, Linn.		N. and Central Europe.
XV.	LIMNEUS, Drap.		
1	stagnalis, Linn.	II., IV.—VIII.	Throughout Europe, Asia, N. America.
2	palustris, Linn.	II.—VIII.	All Europe, N. America.
3	minutus, Drap.	I.—IX.	All Europe, W. Asia, N. Africa, Coquimbo.
4	elongatus, Drap.	II., IV.—VIII.	Europe.
5	pereger, Drap.	I.—X.	All Europe.
5½	*ovatus*, Drap.		

	Genera and Species.	British Distribution.	General Distribution.
$5\frac{1}{3}$	*lineatus*, Bean.		
$5\frac{1}{4}$	*lacustris*, Leach.		
$5\frac{3}{4}$	*acutus*, Jeff.		
6	auricularius, Linn.	II.—VIII.	All Europe, Cachemyr.
7	involutus, Harvey.	VII.	?
8	glutinosus, Mull.	II., IV.	Central Europe.
XVI.	Ancylus, Geoffroy.		
1	fluviatilis, Mull.	I.—IX.	All Europe, N. Africa.
2	lacustris, Mull.	II.—VIII.	All Europe.

Note.—The preceding table is based on Mr. Alder's Catalogue, in the Second Volume of the Magazine of Zoology and Botany. The list has been submitted to him, and I am happy to say the alterations have received his sanction.

Third Report on the Progress of the Hourly Meteorological Register at the Plymouth Dock-Yard, Devonport, Lat. 50° 21′ N. Long. 4° 7′ W., carried on at the request of the British Association, under the direction of Mr. W. SNOW HARRIS, F.R.S.

1. THE Hourly Meteorological Observations, which the Council of the British Association did me the honour to place under my care, have been continued without interruption from the early part of the year 1832. Since that time about 70,000 hourly observations on temperature have been completed, and the results of several years printed in the Reports of the Association for 1835 and 1838.

Similar observations of the barometer and moistened thermometer commenced in January 1837, and briefly mentioned in the Eighth Report of the Association, are now complete for three years. In the present communication I propose to notice some of the principal results of these registers.

The registers of the anemometers invented by the Rev. Mr. Whewell and Mr. Follett Osler, of Birmingham, although carefully attended to, have not been completed for a sufficiently long period, owing to many difficulties and interruptions incidental to the bringing of these instruments into operation. I am not enabled, therefore, to say more at present of Mr. Whewell's instrument than has already appeared in my former Report.

The observations hitherto registered by Mr. Osler's anemometer have been tabulated by him in conjunction with those registered at Birmingham; they will appear in a separate form. We may, however, now look forward to include the results of all the instruments registered here in one general scheme.

Hourly Register of the Barometer.

2. In the three following tables will be found the principal numerical results for the years 1837, 1838, and 1839, comprising the mean hourly and monthly pressures, as also the mean hourly pressures, for the four seasons, and for each six months of summer and winter. From these we may obtain—

1st, The mean pressure.

2nd, The times of the day at which the mean pressure occurs, and the mean daily course of the atmospheric tides, as indicated by the hourly oscillation of the barometric column,

3rd, The mean pressure of different seasons, including spring, summer, autumn and winter, and of summer and winter in periods of six months each.

4th, The course of the atmospheric tides in different seasons, and the times of the day when the mean pressure occurs in each.

5th, The average monthly pressure and periods of the year at which the mean pressure occurs.

6th, The relation between the mean pressure of the twenty-four hours, and that of any given hour or pair of hours.

3. Table I. Plate V.

Containing the Mean Hourly Pressures for each of the Years 1837, 1838, 1839, together with the Mean of these years; from 26,280 observations, at 75 feet above the mean level of the sea, reduced to 32° of Fahrenheit's scale.

The periods of maxima are denoted by the sign +, the minima by the sign −, and the mean by a *.

	1837.	1838.	1839.	Mean of the 3 years
A.M. 1	29·8719	*29·7565	*29·7768	29·8017
2	*29·8696	29·7547	29·7735	*29·7993
3	29·8626	29·7518	29·7688	29·7944
4	29·8608	29·7507	29·7670	29·7928
5	−29·8606	−29·7507	−29·7670	−29·7928
6	29·8619	29·7552	29·7710	29·7960
7	29·8666	*29·7585	29·7755	*29·8002
8	*29·8706	29·7615	*29·7772	29·8032
9	29·8717	29·7637	29·7790	29·8048
10	+29·8732	+29·7645	+29·7807	+29·8061
11	29·8720	*29·7627	29·7788	29·8045
12	*29·8663	29·7587	29·7755	29·8002
P.M. 1	29·8627	29·7540	*29·7705	*29·7957
2	29·8580	29·7517	29·7670	29·7922
3	29·8567	29·7500	29·7657	29·7908
4	−29·8558	−29·7475	−29·7652	−29·7895
5	29·8597	29·7532	29·7685	29·7938
6	29·8629	*29·7557	29·7725	29·7970
7	*29·8679	29·7610	29·7770	*29·8019
8	29·8740	29·7645	*29·7798	29·8061
9	29·8779	29·7672	29·7832	29·8094
10	+29·8792	+29·7665	+29·7840	+29·8099
11	29·8790	29·7665	29·7822	29·8092
12	29·8783	29·7639	29·7775	29·8065
	29·8675	29·7579	29·7743	29·7999

Note.—The * indicating the Mean Pressure is placed between the hours at which the mean occurs when the mean does not correspond to the pressure of either.

Table II. Plate VI. Showing the Mean Hourly Oscillation of the Barometer in the Six Months of Summer and the Six Months of Winter, from 26,280 observations reduced to 32° of Fahrenheit's scale.

Hours.	Summer Months.	Winter Months.
1 A.M.	*29·789	29·813
2	29·785	*29·811
3	29·785	29·806
4	−29·781	−29·804
5	29·781	29·804
6	*29·783	29·809
7	29·788	*29·812
8	29·790	29·816
9	29·791	29·818
10	+29·792	+29·819
11	29·791.	29·818
12	*29·788	*29·812
1 P.M.	29·785	29·806
2	29·781	29·804
3	29·780	29·799
4	−26·778	29·860
5	29·782	−29·805
6	29·785	29·808
7	*29·789	*29·815
8	29·793	29·819
9	29·796	29·824
10	+29·797	+29·822
11	29·796	29·822
12	29·793	29·821
	Mean 29·787	Mean 29·811

Table III. Plate VII. Showing the Mean Hourly Height of the Barometer for each of the Seasons, in periods of Three Months each, reduced to 32°; from 26,280 hourly observations.

Hour.	Spring.	Summer.	Autumn.	Winter.
1 A.M.	*29·824	29·835	*29·743	29·803
2	29·820	*29·830	29·741	*29·803
3	29·814	29·829	29·739	29·799
4	29·814	29·825	−29·737	29·795
5	−29·814	−29·824	29·738	−29·794
6	*29·820	29·826	*29·740	29·798
7	29·824	*29·831	29·745	*29·801
8	29·824	29·832	29·740	29·808
9	29·826	29·831	29·752	29·810
10	+29·826	29·834	+29·750	+29·813
11	29·826	+29·834	29·748	29·810
12	*29·823	29·833	*29·744	*29·801
1 P.M.	29·821	*29·831	29·739	29·792
2	29·819	29·830	29·733	29·787
3	29·815	29·828	29·733	−29·783
4	−29·814	29·825	−29·732	29·787
5	29·816	−29·825	29·739	29·794
6	*29·818	29·827	*29·744	*29·799
7	29·825	*29·829	29·749	29·805
8	29·832	29·835	29·752	29·807
9	29·836	29·839	29·754	29·808
10	+29·836	29·841	+29·753	29·809
11	29·834	+29·841	29·752	29·810
12	29·842	29·836	29·751	+29·810
	29·823	29·831	29·744	29·801

Table IV.

Showing the Mean Monthly Pressures for the Years 1837, 1838 and 1839; together with the Mean of these years, reduced to 32° of Fahrenheit.

Months.	1837.	1838.	1839.	Mean.
January	29·899	29·758	29·929	29·862
February	29·879	29·424	29·933	29·745*
March	29·932	29·751	29·719	29·801
April	29·757	29·774	29·975	29·835
May	29·886	29·763	29·854	29·834
June	29·875	29·757	29·762	29·798
July	29·871	29·899	29·749	29·840
August	29·841	29·848	29·882	29·857
September	29·745	29·858	29·516	29·706
October	30·043	29·887	29·858	29·929+
November	29·832	29·398	29·561	29·597−
December	29·855	29·981	29·557	29·798
Mean	29·8679	29·7581	29·7745	29·800

Horary Oscillation.

4. The results given in Table I. and Plate V. show, that the mean hourly pressure, like that observed between the tropics and in other parts of the world, is subject to a peculiar oscillation, producing two atmospheric tides in twenty-four hours, thereby indicating some general law of our planet of considerable interest, and probably of great practical consequence to Meteorology.

This result is so decided that it becomes apparent with singular regularity through each successive year, in the midst of accidental fluctuations and casual disturbances of very considerable amount. In Table I. will be found collected the mean hourly pressures for the years 1837, 1838 and 1839, together with the mean result of those years reduced to 32° of Fahrenheit by Professor Schumacher's tables. These results are shown on a proportionate scale in Plate V.

5. In laying down the delineations given in this and the succeeding plates, the mean points were first marked off, and then a continuous waving line run through them so as to include the greatest number. It will be found that the deviations are few, and for the most part inconsiderable; almost all the points falling within a continuous and fair curve; where they do not so coincide, the points of observation are denoted by a small star.

6. The mean pressure in this latitude at 32° of Fahrenheit's scale and at 75 feet above the level of the sea, deduced from

26,280 hourly observations, in the three years above mentioned, appears to be 29,800 very nearly.

7. The barometric curve, indicating the atmospheric tides, is on this line four times in the day, viz. at 1·30 A.M., at 7 A.M., at mid-day, or 12 noon, and at 6·30 P.M.

The pressure, having reached a minimum at 4 A.M., continues to ascend for two hours, when it crosses the line of mean pressure at 7 A.M., and ascending for the next three hours, reaches its maximum at 10.

This morning tide* begins now to recede, and again descending for two hours crosses the line of mean pressure at 12 or mid-day; from which, continuing to descend for four hours more, it reaches a minimum at 4. The evening tide begins now to show itself; it ascends for two hours and a half, being on the line of mean pressure at 6·30 P.M., whence, continuing its ascent for three hours and a half, it attains its maximum at 10 P.M. At this point it again turns, and descending for three hours and a half, reaches the point of mean pressure between 12 and 1 A.M., whence it passes in the succeeding two hours and a half to its minimum at 4.

There are consequently, as in the afflux and reflux of the sea, two tides in twenty-four hours.

8. The principal elements of these tides are as follows:

TABLE V.

Morning.		Evening.	
Min.	29·7928 at 4 A.M.	Min.	29·7895 at 4 P.M.
Max.	29·8061 at 10 A.M.	Max.	29·8099 at 10 P.M.
Mean	29·7999 at 7 A.M.	Mean	29·7999 at 6·30 P.M.
Ascending Semi-Oscillation from 4 to 10 A.M.	·0133 Time 6 hours.	Ascending Semi-Oscillation from 4 to 10 P.M.	·0204 Time 6 hours.
Below Line of Mean Pressure	·0071 Time 3 hours.	Below Line of Mean Pressure	·0104 Time 2½ hours.
Above Line of Mean Pressure	·0062 Time 3 hours.	Above Line of Mean Pressure	·0100 Time 3½ hours.

Noon.		Night.	
Time of Mean Pressure, 12.		Time of Mean Pressure, 1·30 A.M.	
Min.	29·7895 at 4 P.M.	Min.	29·7928 at 4 A.M.
Descending Semi-Oscillation from 10 A.M. to 4 P.M.	·0166 Time 6 hours.	Descending Semi-Oscillation, from 10 P.M. to 4 A.M.	·0171 Time 6 hours.
Above Line of Mean Pressure	·0062 Time 2 hours.	Above Line of Mean Pressure	·0071 Time 2½ hours.
Below Line of Mean Pressure	·0104 Time 4 hours.	Below Line of Mean Pressure	·0100 Time 3½ hours.

9. We may distinguish in these results, as delineated in Plate V., four semi-waves, one between 4 and 10 A.M., a second between 10 A.M. and 4 P.M., one between 4 P.M. and 10 P.M., succeeded by one between 10 P.M. and 4 A.M., being precisely

* By the term tide I mean the course of the barometric wave, from the period of its ascent to the termination of its descent, that is, from minimum to minimum.

the critical hours mentioned by M. de Humboldt in his observations in the torrid zone; which is not a little remarkable.

These hours are also mentioned by M. Lamanon, engaged in the voyage of La Perouse.

10. The times of these semi-diurnal variations are therefore equal, but the variations unequal. The ascent = ·0204 in the evening, exceeding the ascent = ·0133 in the morning by about ·0071 of an inch, or one-third nearly.

A similar result is observable in the descending branches; the times are the same, but the fall between 10 A.M. and 4 P.M. = ·0166 is somewhat less than that between 10 P.M. and 4 A.M. = ·0171 by about ·0005. These differences are, however, very small.

On a further examination we find that the two maxima differ by about ·0038 of an inch, that at 10 P.M. being the greatest, and the minima by about ·0033 of an inch. These differences, however, are likewise inconsiderable, the values being nearly the same.

If, therefore, we divide the daily march of the pressure into two complete waves extending through periods of twelve hours each, we observe the wave at night somewhat exceeds the wave by day.

11. The principal elements of this horary oscillation are as follow:

TABLE VI.

Times of Mean Pressure	7 A.M.	12 NOON.	6·30 P.M.	1·30 A.M.
Critical Hours	4 A.M.	10 A.M.	4 P.M.	10 P.M.

Mean Oscillation .... ·01685.

It is not unworthy of remark, that the mean oscillation as above deduced, corresponds nearly with that arrived at by Mr. Daniell from his meteorological register near London, the numbers being ·0168 and ·0150, a difference not probably greater than might be expected from the nature of the observations and difference of latitude.

Horary Oscillation, &c., in Different Periods of the Year.

12. In Plate VI. will be found delineated the hourly march of the pressure through a mean day of summer and winter, divided into periods of six months each. The period of summer being from May to October inclusive—the period of winter from November to April inclusive. The following Table contains the general results as deduced from Table II.:

Table VII.

	Summer.				Winter.			
Mean Pressure	29·787				29·811			
	A.M.		P.M.		A.M.		P.M.	
	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.
Time of Mean Pressure	1 30	7 0	0 25	6 30	2 0	7 0	noon.	6 30
Critical Hours	4 0	10 0	4 0	10 0	4 0	10 0	3 0	9 0
Mean Oscillation	·015				·020			
Differences from Mean of Year	·0018 –				·0031 +			

The mean oscillation of winter by this table appears greater than that of summer, whilst the critical hours of evening fall an hour sooner.

13. In Plate VII. is delineated from Table III. the hourly march of the pressure through a mean day of spring, summer, autumn and winter, divided into periods of three months each, and in the following Table are given the principal elements of these oscillations.

Table VIII.

Spring.				Summer.				Autumn.				Winter.			
Mean Pressure 29·823				Mean Pressure 29·831				Mean Pressure 29·744				Mean Pressure 29·801			
Times of Mean Pressure.															
A.M.		P.M.		A.M.		P.M.		A.M.		P.M.		A.M.		P.M.	
. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.
1 25	6 45	0 15	6 30	2 0	7 10	1 40	7 20	1 0	7 0	noon	6 0	2 30	7 0	noon	6 15
Critical Hours.															
[illegible] 0	10 0	4 0	10 0	5 0	10 0	4 0	10 0	4 0	9 0	4 0	10 0	5 0	10 0	3 0	11 0
Mean Oscillation.															
·017				·013				·018				·023			
Differences from Mean Oscillation of the Year.															
·0001 +				·0038 –				·001 +				·0061 –			

It appears by this table that the mean oscillation of winter is the greatest, and that of summer the least; whilst the mean oscillation of spring is not very different from that of the whole year, the oscillation of autumn being a mean between summer and winter.

14. Although the times of the horary semi-oscillations in the different seasons, as shown in Plates V. and VI., are equal, or nearly so, yet the oscillations themselves are unequal, as in the former instances for each year delineated in Plate V.; the ascent in the evening generally exceeding the ascent in the morning, except in the period of the three months of winter laid down in Plate VII., when the reverse appears to be the case, a circumstance by no means unworthy of notice.

The difference in the amount of the morning and evening waves, as shown in Plate VII., is greatest in summer and spring, and least in autumn and winter.

15. Supposing, as before, the daily march of the pressure to consist of two complete waves, extending through periods of twelve hours each, then the comparative amount of these may be seen in the three following tables:

Table IX.

Showing the Mean Morning and Evening Oscillation in the Years 1837, 1838, 1839, with their Differences from the Mean of each year; together with the Difference of the Morning and Evening Mean Oscillations.

Years.	Mean Oscill. of Year.	A.M.		P.M.		Difference of Morning and Evening.
		Oscillation.	Difference.	Oscillation.	Difference.	
1837	·0180	·0150	·003−	·0210	·003+	·0060
1838	·0164	·0154	·001−	·0174	·001+	·0020
1839	·0162	·0146	·0016−	·0179	·0016+	·0033
Mean ...	·0168	·015	·0018−	·0187	·0018+	·0038

Table X.

Showing the Mean Morning and Evening Oscillation of Summer and Winter in Periods of Six Months each, with their Differences from the Mean Oscillation of each Season, and of the Year; also the Differences of the Morning and Evening Mean Oscillations.

Seasons.	Mean Oscillation of Seasons.	A.M.			P.M.			Differences of A.M. & P.M.
		Oscillation.	Differences.		Oscillation.	Differences.		
			Season.	Year.		Season.	Year.	
Summer .	·015	·0125	·0025−	·0043−	·0175	·0025+	·0026+	·005
Winter...	·020	·0175	·0025−	·0006	·0225	·0025+	·0056+	·005

Table XI.

Showing the Mean Morning and Evening Oscillations of Spring, Summer, Autumn and Winter, in Periods of three Months each, with their Differences from the Mean Oscillation of each Season, &c., of the Year; also the Difference of the Morning and Evening Mean Oscillations.

Seasons.	Mean Oscillation of Seasons.	A.M.			P.M.			Differences of Morning and Evening.
		Oscillation.	Differences.		Oscillation.	Differences.		
			Season.	Year.		Season.	Year.	
Spring ...	·0170	·0120	·005 –	·0048 –	·0220	·005 +	·005 +	·010
Summer .	·0130	·0095	·003 –	·0073 –	·0165	·003 +	·0003 –	·007
Autumn .	·0185	·0175	·001 –	·0006 +	·0195	·001 +	·0026 +	·002
Winter...	·0230	·0245	·0015 +	·0077 +	·0215	·0015 –	·0047 +	·003

16. The differences in the amount of the morning and evening waves appear to be so regularly developed, that I cannot doubt of its being included in some general law of the daily pressure. The fact receives further confirmation from the observations recorded by Lieut.-Col. Sykes, in his valuable paper on the Meteorology of Dukhun*, by which it appears that the amount of the ascent of the barometer, between 4 P.M. and 10 P.M., is generally greater than that between 4 A.M. and 10 A.M.

I find however by these observations, that the amount of the wave is increased at night rather by the previous depression than by the following maximum, since the pressure has been seldom observed to attain so high a point at night as that from which it descended in the morning. In our observations, however, the maximum at 10 P.M. is generally greater, by a small quantity, than the maximum at 10 A.M., except in the three months of winter already mentioned. The accuracy of this result we have no reason to doubt; for, besides that the observations have been reduced to 32°, the temperature of the respective times of observation, viz. 10 A.M. and 10 P.M., did not differ by above ·4 of a degree.

17. It may appear, however, worth while to consider how far a difference of temperature at 4 A.M. and 4 P.M. may, by operating on the mercurial vapour, cause an undue depression of the column at the latter hour; but this source of error is decidedly negatived by the results given in Table II., Plate II., for the months of summer, in which we find the amount of depression at 4–5 A.M. greater than at 4–5 P.M. I am content therefore, at

* Philosophical Transactions for 1835.

present, merely to register the fact, and to collect in the following table a few examples of the differences observed in the semi-diurnal and nocturnal tides at a few places where they appear to have been most accurately determined, and which I have derived principally from Lieut.-Col. Sykes's valuable paper on the Meteorology of Dukhun*.

Table XII.

18. Showing the Differences in the Morning and Evening Tides at various places, with the Difference from the Mean.

Place.	Mean Oscillation.	Morning Tides. Ascending +		Morning Tides. Descending −		Evening Tides. Ascending +		Evening Tides. Descending −	
		4-5 to 9-10 A.M.	Difference.	9-10 A.M. to 4-5 P.M.	Difference.	4-5 to 9-10-11 P.M.	Difference.	10-11 P.M. to 4-5 A.M.	Difference.
Madras ...	·0560	·0470	·009 −	·0790	·023 +	·0630	·007 +	·0350	·021 −
London ...	·0227	·0185	·004 +	·0289	·006 +	·0272	·004 +	·0162	·006 −
Poona	·0656	·0445	·021 −	·1116	·046 +	·0884	·022 +	·0181	·047 −
Plymouth	·0168	·0133	·0035 −	·0166	·0002 −	·0204	·0036 +	·0171	·0003 +

Difference of A.M. and P.M.	Ascending +	Descending −
Madras	·016	·0440
London	·008	·0127
Poona	·044	·0935
Plymouth	·007	·0005

We may perceive by this table that the differences in the morning and evening semi-oscillations may be very considerable. At Poona, for example, the afternoon ascending tide was nearly twice as great as the morning ascending tide; in order therefore to fix with precision the amount of the barometric oscillation in different places, we require in the present state of our knowledge of this question, at least an observation at each of the critical hours, since the mean result in column 2. may differ considerably from the others, especially in a limited series of observations.

19. For perfect experimental deductions, nothing short of a long series of continued hourly observations will in the present state of this question suffice, since we are quite unacquainted with the fluctuations which may possibly occur at different periods and seasons, and in different states of the pressure; all the observations therefore hitherto made, although extremely valuable, must still be considered in this sense comparatively deficient.

* Transactions of the Royal Society for 1835. Part I.

20. Thus the results given in Table XII. cannot be regarded as strictly comparable, the observations having been in some instances confined to short periods of time, also to different seasons, and to certain portions of the day, in which the difference from the mean oscillation of the twenty-four hours has never yet been ascertained. In order to make such observations completely available, it is requisite to determine these differences in different places, and at different altitudes above the sea, not only for a mean year, but for different seasons, and even months. We have not, however, at present, data competent to such deductions.

21. I have arranged in the three following tables such information on this point as is derivable from the extensive series of hourly observations now under consideration, and which will be found to contain the amount of the four semi-oscillations in different years and seasons, with their various differences from the mean of each.

Table XIII.

Showing the four Semi-Oscillations of the Years 1837, 1838, 1839, with the Difference of each from the Mean Oscillation of the Year, together with the Differences of the Two Ascending and the Two Descending Oscillations.

Years.	Mean Oscillation.	Morning Tides.				Evening Tides.			
		Ascending +		Descending −		Ascending +		Descending −	
		4 to 10 A.M.	Difference.	10 A.M. to 4 P.M.	Difference.	4 to 10 P.M.	Difference.	10 P.M. to 4 A.M.	Difference.
1837	·01800	·0126	·0054 −	·0174	·0006 −	·0234	·0054 +	·0186	·0006 +
1838	·01640	·0138	·0026 −	·0170	·0006 +	·0190	·0026 +	·0158	·0006 −
1839	·01625	·0137	·0025 −	·0155	·0007 −	·0188	·0025 +	·0170	·0007 +
Mean	·01688	·01336	·00352 −	·01663	·00025 −	·0204	·00352	·01713	·00025 +

	Years.	Ascending +	Descending −
Differences of A.M. and P.M.	1837	·0108	·0012
	1838	·0052	·0012
	1839	·0051	·0015
	Mean	·007	·0005

Table XIV.

Showing the four Semi-Oscillations in Summer and Winter in periods of Six Months each, with the Differences of each from the Mean of the Whole Year, together with the Differences of the two Ascending and the two Descending Oscillations.

Seasons.	Mean Oscillation.	Morning Tide.				Evening Tide.			
		Ascending.		Descending.		Ascending.		Descending.	
		4 to 10 A.M.	Difference.	10 A.M. to 4 P.M.	Difference.	4 P.M. to 10 P.M.	Difference.	10 P.M. to 4 A.M.	Difference.
Summer.	·015	·011	·006−	·014	·003−	·019	·002+	·016	·0008
Winter..	·020	4 to 10 A.M. ·015	·002−	10 A.M. to 3 P.M. ·020	·003+	3 P.M. to 9 P.M. ·025	·008	9 P.M. to 4 A.M. ·020	·003

	Seasons.	Ascending +.	Descending −.
Difference of A.M. and P.M.	Summer	·008	·002
	Winter	·010	·000

Table XV.

Showing the four Semi-Oscillations of Spring, Summer, Autumn and Winter, in Periods of Three Months each, with the Difference of each from the Mean of the Whole Year, together with the Differences of the two Ascending and the two Descending Oscillations.

Seasons.	Mean Oscillation.	Morning Tides.				Evening Tides.			
		Ascending +	Difference.	Descending −	Difference.	Ascending +	Difference.	Descending −	Difference.
Spring .	·017	4 to 10 A.M. ·012	·005−	10 A.M. to 4 P.M. ·012	·005−	4 to 10 P.M. ·022	·005+	10 P.M. to 4 A.M. ·022	·005
Summer	·013	5 to 10 A.M. ·010	·007−	10 A.M. to 5 P.M. ·009	·008−	5 to 10 P.M. ·016	·008−	10 P.M. to 5 A.M. ·017	·000
Autumn	·0185	4 to 9 A.M. ·015	·002−	9 A.M. to 4 P.M. ·020	·003+	4 P.M. to 9 P.M. ·022	·005+	9 P.M. to 4 A.M. ·017	·000
Winter .	·023	5 to 10 A.M. ·019	·002+	10 A.M. to 3 P.M. ·030	·013+	3 to 11 P.M. ·027	·010+	11 P.M. to 5 A.M. ·016	·000

	Seasons.	Ascending +.	Descending −.
Difference of A.M. and P.M.	Spring	·010	·010
	Summer	·006	·008
	Autumn	·007	·003
	Winter	·008	·014

The results in the three last tables point out, 1st, That the descending semi-oscillations from 10 A.M. to 4 P.M., and from 10 P.M. to 4 A.M., approach nearest the mean oscillation of the periods to which they belong.

Thus in Table XIII. the descending oscillations at these hours differ but little from the mean oscillation of the respective years:—the same is true in Tables XIV. and XV. The descending tides differ but little from the mean of their respective seasons;—these differences, however, are not expressed in the tables.

2nd. It may be perceived that the mean of the two ascending or two descending oscillations is exactly the mean oscillation of the periods to which they belong.

Hence the differences from the mean are equal, and in opposite directions; thus in the semi-oscillations for summer (Table XIV.), the morning descending oscillation = ·014 is as much below the mean oscillation ·015, as the evening descending oscillation = ·016 is above it.

3rd. That the ascending tide from 5 to 10 P.M. of summer (Table XV.), approaches very nearly the mean of the year.

4th. That the descending tides, from 9–10 P.M. to 4 A.M. of summer (Table XIV.), and of summer, autumn and winter (Table XV.), approach also very nearly to the mean oscillation of the year. Hence, if only one set of observations can be made, and those in the day time, it would be desirable to register them during the three months of summer at 4 P.M. and 10 P.M. If two sets of observations at the critical hours can be made, then it would appear desirable to choose either of the periods of the two ascending or the two descending tides, and take the mean of the two, as the mean of the period. If these be continued for a whole year, we thence obtain the mean oscillation of the year;—if for any season, the mean oscillation of that season:—whence, knowing the difference from the mean oscillation of the whole year, we may apply the requisite correction.

So far as these observations extend therefore, we may infer, that four observations in the day in the three months of spring, viz. at 4 and 10 A.M., and 4 and 10 P.M.; or, otherwise, at 10 A.M. and 4 P.M., at 10 P.M. and 4 A.M., would give a mean result, differing from the mean oscillation by a very small quantity.

There is little doubt, as suggested by Major Sabine, in his Register of the Barometer at Port Royal*, Jamaica, that the amount of the horary oscillation varies with some function of

* Daniell's Meteorology.

the distance from the equator; and Professor Forbes, in his excellent paper on the Horary Oscillations of the Barometer near Edinburgh, has given a formula which applies with considerable accuracy to such observations as have been yet made; and although, as we have seen, considerable variations from the mean may arise from the times and seasons of such observations, nevertheless the author has evinced admirable philosophical tact in his method of treating the subject, and has fortunately selected for his term of comparison the semi-mean oscillation between 10 and 4 P.M., which perhaps, upon the whole, may be found to give a tolerably fair approximation. The question, however, must still be considered in its infancy, and but little investigated by the powerful aid of contemporary and long continued hourly observations in different latitudes. It would therefore, at present, be useless to enter upon a discussion of the anomalies alluded to by Professor Forbes in the observations hitherto made at different places. Thus, at Paris the minimum oscillation was observed to take place in summer, whilst, by Professor Forbes's deductions, it occurred in winter. The results now under discussion give the minimum also in summer, but the maximum in winter. We require, therefore, a very accurate series of observations continued hourly for a long time, in order to reconcile such discrepancies, and bring us fairly acquainted with the course and nature of these atmospheric tides.

It may not be uninteresting, in concluding for the present this part of the discussion, to tabulate a few of the best authenticated observations of the barometric oscillations in different parts of Great Britain.

TABLE XVI.

Place.	Altitude above Sea in feet.	Lat. N.	Horary Oscillations			Mean Oscillation as deduced from the Two − Oscillations.
			Morning −	Evening +	Night −	
			10 A.M. to 4 P.M.	4 P.M. to 9, 10, 11 P.M.	9, 10, 11 P.M. to 4 A.M.	
Plymouth	75	50·21	·0166	·0204	·0171	·0168
London ...	95	50·51	·0280	·0230	·0166	·0220
York	35	53·56	·0174	·0170	·0094	·0134
Edinburgh	430	55·55	·0106	·0097	——	——

The mean oscillation deduced from London in the above table will, I apprehend, upon the whole, be found too great, owing possibly to the want of a sufficient number of observations, especially at 4 and 10 A.M. If we substitute for it Professor

Daniell's result, ·015, and take the morning negative oscillation for Edinburgh = ·0106 as a near approximation to the mean, then we have the following series :—

Table XVII.

Latitude.	50 . 21	50 . 51	53 . 56	55 . 55
Oscillation	·0168	·0150	·0134	·0106

By which we perceive that the amount of the oscillation is less in the greater latitudes.

Professor Forbes's formula $\cdot 1193 \cos^{\frac{5}{2}} \theta - \cdot 0150$, &c., cannot exactly correspond with these numbers, except the last, since his index of the cos . θ was obtained by selecting observed oscillations between 10 and 4 A.M. According to this formula we should have for the oscillations at these latitudes,

Plymouth.	London.	York.	Edinburgh.
·0238 ;	·0227 ;	·0167 ;	·0130.

Now the formula gives a fair approximation to the evening oscillations at these places, which are—

Plymouth.	London.	York.	Edinburgh.
·0204 ;	·0230 ;	·0170 ;	·0097.

It may be further remarked as a curious coincidence, that the night oscillation at London corresponds with the morning oscillation at Plymouth, whilst the morning oscillation at York corresponds nearly with the night oscillation at Plymouth.

Average Monthly Pressure.

In Table IV. we have given the average monthly pressure for the years 1837, 1838, 1839, together with the mean of these years, by which it will be perceived that the annual variations have not for this period of time been at all regular.

Thus in the year 1837 the maximum occurred in October, and the minimum in September, the mean about July, whilst in 1838 the maximum occurred in July, the minimum in November, and the mean in January ; in 1839 the maximum occurred in April, the minimum in September, and the mean about June.

The table, however, upon the whole, indicates a maximum and minimum towards the close of the year, and a mean in spring, which perhaps will become more apparent by an increased number of observations.

In the following table will be found the deviations from the mean of the three years through the different months, in which

it will be seen that the greatest deviations occurred in October and November, and the least in March.

Table XVIII.

Showing the Deviations of the Mean Monthly Pressure for the Mean for the Years 1837, 1838, 1839, reduced to 32° of Fahrenheit.

Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
+·062	−·055	+·001	+·035	+·034	−·012	+·040	+·057	−·094	+·129	−·203	−·002

The months of March and December, therefore, approach nearest the mean, whilst the maximum and minimum occur in October and November.

The recurrence of the mean pressure four times in the twenty-four hours at once points out the hours, or pairs of hours, which would be most likely to give the mean pressure of the twenty-four, since it would occur either at the stated hours themselves, or between those falling on different sides of them.

In the following table will be found the deviation of each hour from the mean, and in the next table the pairs of hours, the mean pressure of which approach nearest the mean of the whole twenty-four.

Table XIX.

Showing the Deviation of each Hour from the line of Mean Pressure.

A.M.		P.M.	
Hours.	Deviation.	Hours.	Deviation.
1	·0018 +	1	·0042 −
2	*·0006 −	2	·0077 −
3	·0055 −	3	·0091 −
4	·0071 −	4	·0104 −
5	·0071 −	5	·0061 −
6	·0039 −	6	·0029 −
7	*·0003 +	7	*·0020 +
8	·0032 +	8	·0062 +
9	·0049 +	9	·0095 +
10	·0062 +	10	·0100 +
11	·0046 +	11	·0093 +
12	*·0003 +	12	·0066 +

By this table it appears, 1st, that the single hours, which give the nearest approximation to the mean, are 7 A.M. and 12 at noon, next to these 2 A.M. and 7 P.M.; 2nd, that the mean

annual pressure of any hour does not differ more than about ·01 from the mean annual pressure of the twenty-four hours.

We may hence render any past register at Plymouth available in deducing the mean pressure, by applying, according to the signs, the corrections given in the preceding table.

Table XX.

Showing the Pairs of Hours, the Mean Pressure of which approaches nearest the Mean of the Twenty-four Hours.

Pairs of Hours.	8 A.M. and 6 P.M.	3 A.M. and 8 P.M.	9 A.M. and 1 P.M.	3 P.M. and 10 A.M.	4 A.M. and 10 A.M.	9 A.M. and 5 P.M.	2 P.M. and 8 P.M.	11 A.M. and 6 P.M.	2 P.M. and 9 P.M.	6 A.M. and 7 P.M.
Deviations	·0001+	·0003+	·0003+	·0004+	·0005−	·0006−	·0007−	·0008+	·0009+	·0010−

The mean pressure therefore at Plymouth may be obtained by observations at either of the pairs of hours above given.

It may be further seen by Table I. that the mean maximum and mean minimum pressures are 29·8080, and 29·7911, giving a mean oscillation = ·0169, or very nearly the same as that = ·01688 already deduced from the four same oscillations, Table VI.

It is not therefore unimportant to determine the hours which either give exactly or come nearest the mean maxima and minima of the twenty-four.

Now the pressure at 3 P.M. differs extremely little from the mean minimum, the difference being only ·0003, whilst the mean pressure at 11 P.M. approaches nearest the mean maximum, the difference being only ·0012. If we had taken these observations at these hours as the mean maxima and minima of the twenty-four, we should have had for the mean oscillation ·0184, which would have differed from the true mean oscillation by ·0015, and for the mean pressure 29·8000, which would have been very near the true mean pressure.

The mean results given in Table I. enable us to find the single hours, or pairs of hours, which give either singly or combined the mean maxima and minima pressures, or approach nearest to them. These are given in the next table.

TABLE XXI.

Showing the Hours which, either singly or combined, approach the Mean Maxima and Minima Pressures.

		Single Hours.			Double Hours.			
Mean Maximum Pressure. 29·8080.	Hour.	8 P.M.	9 P.M.	11 P.M.	10 A.M. and 10 P.M.	8 P.M. and 10 P.M.	11 P.M. and 12 P.M.	11 P.M. and 8 P.M.
	Deviations	·0019−	·0014+	·0012+	·000	·000	·0002	·0004
Mean Minimum Pressure. 29·7911.	Hour.	3 P.M.	2 P.M.	4 P.M.	4 A.M.	5 P.M.	3 A.M.	4 A.M. and 4 P.M.
	Deviations	·0003−	·0011+	·0016−	·0017+	·0027+	·0033+	·000−

We may infer from this table that three observations at 3 P.M., and again at 8 and 10 P.M., would give us the mean minima and mean maxima pressures very nearly; consequently the mean pressure and the mean oscillation = ·0172, which differs from the true mean oscillation by only ·0003 +. The remaining hours may afford approximation by applying corrections according to the signs.

I regret that it is not in my power to give the results of any good discussion of the force of the wind for each hour without regard to direction, as I have reason to believe it would throw some light on the phænomena of the horary oscillation. Thus I have little doubt that the suppression of the morning oscillation observed in the months of summer and spring in Plate 3 is coincident with the force of the wind, which, from a cursory examination of the indications of Osler's anemometer, appears to be in those seasons greatest at those periods. I hope in a future Report to be enabled to put the Association in possession of some further information on this point.

The mean hourly observations of pressure are included in Plate 4, with those on temperature, and the dew point, as determined from the indications of the moistened thermometer by Professor Apjohn's formula. The following table contains the Mean Results of the latter for the Years 1837, 1838, 1839.

TABLE XXII.

Hour.	Thermom.	Hygrometer.	Difference.	Dew-Point.
1 A.M.	47·52	46·20	1·32	45·00
2	47·33	46·03	1·30	44·75
3	47·11	45·92	1·19	44·75
4	47·00	45·66	1·34	44·25
5	46·98	45·77	1·21	44·75
6	47·41	46·01	1·40	44·50
7	48·44	46·83	1·61	45·25
8	49·68	47·51	2·17	45·00
9	51·30	48·50	2·80	35·26
10	52·84	49·45	3·39	46·25
11	53·00	50·02	3·88	46·75
12	54·51	50·40	4·14	46·75
1 A.M.	45·83	50·55	4·28	46·75
2	54·77	50·44	4·33	46·75
3	54·25	50·24	4·01	46·75
4	53·45	49·80	3·65	46·75
5	52·27	49·06	3·21	46·25
6	51·24	48·46	2·78	46·00
7	50·28	47·90	2·38	45·75
8	49·44	47·51	1·93	45·75
9	48·83	47·17	1·66	45·60
10	48·48	46·93	1·55	45·60
11	48·10	46·66	1·44	45·00
12	47·80	46·43	1·37	45·00
Mean	50·32	47·89	2·43	45·60

It appears by these results,

1st. That the greatest dew point does not exceed 47 degrees or the point of minimum temperature.

2nd. That the least dew point does not fall below 44 degrees, and occurs about the time of minimum temperature, viz. at 4, 5, 6, A.M.

3rd. That the mean dew point = 45·6 differs from the mean temperature = 50·3 by about 5 degrees, which may therefore be taken as the mean dryness on the thermometric scale.

4th. That the mean dew point occurs twice in the twenty-four hours, viz., about 9 A.M. and 9 P.M., the interval being nearly twelve hours.

5th. The mean temperature of the dew point approaches very nearly the mean minimum temperature of the wet bulb thermometer.

These results are, so far as I know, the first which have been

derived from Dr. Apjohn's formula, applied to an extensive series of hourly observations.

When we consider of how great importance it is to Meteorology to perfect the method of hygrometric observations by the moistened thermometer, I cannot but hope that those approximations may lead to future and more accurate results. Having found the formula in very many instances give close approximations to the dew point, as determined by the direct method of Daniell, I cannot but think that it will eventually become an important acquisition to this department of science.

The very recent completion of the numerical results of these first years' hourly observations on the barometer and dew point necessarily limits this Report to some of the more immediate deductions which they present. It is further to be observed that the weather of these years has been of a disturbed and unsettled character; especially the last two, which have abounded in heavy rain and hard gales. Comparing the easterly and westerly winds (leaving the due north and south out of the account), I find that the west winds have blown for one-half the whole period, and the easterly for one-quarter only. The ratio being nearly 2 : 1. Comparing the northerly with the southerly winds (leaving the due east and west out of the account), the latter have blown for one-fourth of these three years, and the former for one-eighth; the ratio being also about 2:1. The great prevalence of the southerly and westerly over the northerly and easterly winds has been accompanied by a mean pressure probably below what will appear in the mean of a greater number of years. In these years there occurred twenty-eight hard gales, principally from S.E. to N.W.

Horary Observations on Temperature.

Since the discussion of the observations on temperature, two valuable communications have been placed in my hands; the first by Professor Bache, which contains an hourly register of the thermometer from June 1837 to June 1838, at Frankford Arsenal, by Captain Alfred Mordecai, of the United Service Corps of Ordnance. The second contains three hourly registers for the year 1837, by Major Ord, of the Royal Engineers, at the three most important meteorological and geographical stations in the island of Ceylon, viz., Colombo, Kandy, and Trincomalee.

I am enabled from these registers to extend the table given in page 199 of the Fifth Report of the Association, so as to include these places.

Table XXIII.

Shewing the Times of Morning and Evening Mean Temperature in different Latitudes, and at various Heights above the Sea.

Name of Place.	Latitude N.	Longitude.	Height above Sea.	Distance from Sea.	Mean Temperature.	Time of Morning Mean.	Time of Evening Mean.	Interval between Morning & Evening Mean.
			Feet.					
Leith	55·56	3·13 w.	25	600	48	9·13	8·27	11·14
Plymouth	50·21	4· 6 w.	75	400	52	8·	7·	11·
Padua...............	45·36	11·55 E.	——	——	——	8·41	7·52	11·14
Philadelphia	39·57	75· 9 w.	——	——	49·28	8·10	7·30	11·20
Colombo	6·57	80· E.	36	near	80·16	10·35	9·30	10·55
Kandy	7·18	80·49 E.	1682	——	74· 5	10·	9·	11·
Trincomalee	8·33	81·24 E.	60	——	81·	10·35	8·40	11· 5

This table is of much value, since it contains the results of a series of observations on temperature, calculated to establish the two daily periods, the mean temperature of which equals the mean temperature of the twenty-four hours.

It may be seen by this table that notwithstanding some variations occur in the times, yet, as observed by Professor Forbes in his excellent Report on Meteorology, the interval between the morning and evening mean is upon the whole remarkably constant, especially when we consider the difficulty of obtaining hourly observations for a great length of time, so as to completely neutralize every casual disturbance.

Major Ord was led to adopt a two-hourly register in preference to an hourly register, from a conviction that an equal if not a greater degree of accuracy would be obtained in that particular climate.

The register was carried on by the intelligent Scots of the 98th Native Highlanders, and non-commissioned officers of the Royal Artillery.

Thus the fine efforts of an hourly register, first commenced and discussed under the direction of Sir D. Brewster at Leith, in the years 1824 and 1825, have been, and will doubtless still continue to be, followed by similar registers, the results of which cannot fail to be of the highest value to Meteorology, as being the only channel through which any specific practical information can be obtained in this most valuable department of physics.

Whatever therefore may be the claims of further inquirers on the consideration of the scientific world, it must never be forgotten that Sir David Brewster was the first to obtain an

hourly meteorological register for a series of years, and to enrich this branch of science with many important facts which such a register under the powerful scrutiny of his philosophical mind would necessarily supply*.

Plymouth, August 10, 1839.

* The remaining observations of the year 1839 have been combined with the others and discussed since this Report was presented to the Physical Section of the Association at its last Meeting at Birmingham.

On the action of Air and Water upon Iron, &c.

Mr. Mallett reported the progress of the investigation on his subject, which was entrusted to Professor E. Davy and himself, and stated his expectation, that a full Report of the Results of the experiments would be presented to the next Meeting of the Association. (See Report for 1838.)

Report on a Machine for the detection and measurement of Gases present in small quantities in Atmospheric Air, Coal-Gas, &c. By WILLIAM WEST.

This instrument consists of a gasmeter turned slowly round by clockwork, so as to draw the air, or other mixture of gases under examination, through liquids proper to combine with and detain the gases sought, as solution of lime or barytes for carbonic acid, a dilute acid for ammonia, a salt of lead for hydrosulphuric acid, &c. The hands on the clock-face denote the volume of air thus submitted to partial analysis, and from the weight of the new compound formed in the liquids the proportion of foreign gases separated may be obtained by calculation.

The advantage of this apparatus over the former modes of attempting the same object, consists in the large quantity which can thus be examined, several hundred cubic feet for instance, instead of a few inches. It is intended to apply the apparatus to the examination of the air of towns, by simultaneous experiments with machines in the town and the adjacent country, some of these to be at different distances and variously placed as to the direction of the wind. These experiments will require longer time and additional apparatus. The principle has been successfully applied to measure the proportions of sulphuretted hydrogen, and of ammonia in coal-gas.

On the subject of a resolution adopted by the Meeting of the British Association held at Newcastle, in August 1838, to the following effect:—

"Resolved,—That it is desirable that the whole of the stars observed by Lacaille at the Cape of Good Hope, the observations of which are recorded in his *Cœlum Australe Stelliferum*, should be reduced.

"That Sir J. Herschel, Mr. Airy, and Mr. Henderson, be a Committee for carrying the same into effect, and that 200*l.* be appropriated for the purpose."

The Committee report,—That considerable progress has been made in the reduction of the stars in Lacaille's *Cœlum Australe Stelliferum*; and that although only a small portion of the money appropriated has been actually expended, nearly the whole will probably be required during the ensuing year to complete the work.

On the subject of a resolution agreed to by the British Association at their Meeting at Newcastle, in August 1838, to the following effect:—

"Resolved,—That it is desirable that a revision of the nomenclature of the stars should be made, with a view to ascertain whether or not a more correct distribution of them amongst the present constellations, or such other constellations as it may be considered desirable to adopt, may be formed.

"That Sir J. Herschel, Prof. Whewell, and Mr. Baily, be a Committee for that purpose, and do report on the same at the next Meeting of the Association.

"That the sum of 50*l*. be appropriated to defray any expenses that may be incurred in this inquiry."

Your Committee report,—That some progress has been made in reforming the nomenclature of the northern constellations, and that they have commenced laying down the stars in the southern on a planisphere, according to their observed actual magnitudes, for the purpose of grouping them in a more convenient and advantageous manner. No expense has yet been incurred in this inquiry; but the Committee are desirous that the grant should be continued for another year.

On the subject of a resolution adopted by the Meeting of the British Association held at Newcastle, in August 1838, to the following effect:—

"Resolved,—that Sir J. Herschel and Mr. Baily be requested to make application to Government for increase in the instrumental power of the Royal Observatory at the Cape of Good Hope, and the addition of at least one assistant to that establishment."

The Committee report,—That application had already been made to Government previous to the date of this resolution, viz. on the 29th June, 1838, and on other occasions, to the effect therein mentioned; and that in consequence of those applications, aided no doubt by a knowledge of its being the wish of the British Association, (such wish having been communicated to Lord Minto by Sir J. Herschel in a letter dated Nov. 5, 1838,) they have great pleasure in being able to state,—

1st. That — Mann, Esq., has been appointed as an addi-

tional assistant to the Cape Observatory at a liberal and sufficient salary, and is already on his voyage thither to take on himself the duties of his office.

2nd. That Jones's mural circle, hitherto used at Greenwich, has been despatched by the orders of the Lords Commissioners of the Admiralty to supply the place of the defective one in use up to that time at the Cape, and is already probably arrived there,—an improvement of essential importance, the Greenwich instrument having been shown, by many years' trial in the hands of Messrs. Pond and Airy, to be of the highest excellence. The liberality of Mr. Airy in resigning this noble instrument to his brother astronomer cannot, in your Committee's opinion, be too highly estimated.

3rd. That for the purpose of enabling Mr. Maclear, the director of the Cape Observatory, to prosecute with effect the remeasurement of Lacaille's arc of the meridian at the Cape, the Lords Commissioners of the Admiralty have procured from the Board of Ordnance the use of Colonel Colby's excellent compensation-measuring bars, the same which have been employed by him in the measurement of the Irish base, which are now actually on their voyage to perform a similar office at the Cape.

4th. That the Lords Commissioners of the Admiralty have, in pursuance of the same object, applied for and obtained the use of an excellent theodolite, the property of the Royal Astronomical Society, and by them liberally granted for the purpose of remeasuring Lacaille's triangles.

On the subject of a resolution adopted by the Meeting of the British Association at Newcastle, in August 1838, to the following effect:—

"That Sir J. Herschel be requested to superintend the Reduction of Meteorological Observations made, agreeably to his recommendation, at the Equinoxes and Solstices, and that 100*l.* be placed at his disposal for that purpose."

Sir J. Herschel reports,—That he has, within the course of the year elapsed since the last Meeting of the Association, received several series of observations from distant stations, completing wholly or in part the series before transmitted from those stations; but that several are still deficient, which, however, must now be considered either as irrecoverably lost, or as never having been made. That, partly owing to the comparative inutility of reducing incomplete series, and partly to the pressure of other business, especially to that arising from the Magnetic Expedition and observatories, which will form the subject of a distinct report, he has been prevented hitherto from

making material progress in preparing the observations in question for reduction, but hopes before the next Meeting to get them executed; and requests the grant of the Association may be continued for that purpose, and that the surplus, if any, may be made applicable to the reduction of other meteorological observations communicated to him while resident at the Cape and since, and not made at the solstices and equinoxes, but in the form of monthly registers.

On a grant of 500*l.* made for the reduction of stars in the *Histoire Céleste*, under the superintendence of Mr. Baily, Mr. Airy, and Dr. Robinson,

Mr. Baily reports,—That the reduction of stars in the *Histoire Céleste* has been commenced, and already about 13,000 stars have been reduced at an expense of about 170*l.* It is presumed that the greater part (if not the whole) of the remainder may be completed in the course of the ensuing year, and it is therefore expedient that the grant of money should be continued.

On a grant of 500*l.* made for the purpose of extending the catalogue of the stars of the Royal Astronomical Society, under the direction of Mr. Baily, Mr. Airy, and Dr. Robinson,

Mr. Baily reports,—That with respect to the extension of the Astronomical Society's catalogue of stars, about one half of the computations are completed in consequence of the liberal grant from the British Association at Liverpool, and about 180*l.* of that grant has been expended; the whole of the remainder of the grant will probably be required before the next Meeting of the Association.

The Committee appointed at Newcastle (in Report for 1838, p. xxiii.) for the purpose of applying to Government for a proper place for the deposit of records connected with the mining transactions of Great Britain, reported that a room adjoining the Museum of Economic Geology had been appointed for their reception, which would be placed under the custody of a proper keeper.

The Committee appointed at Newcastle (in Report for 1838, p. xxv.), superintending Inquiries into the Statistics of the Collieries of the Tyne and Wear, presented a notice of the progress of their researches.

NOTICES

AND

ABSTRACTS OF COMMUNICATIONS

TO THE

BRITISH ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE,

AT THE

BIRMINGHAM MEETING, AUGUST, 1839.

ADVERTISEMENT.

The Editors of the following Notices consider themselves responsible only for the fidelity with which the views of the Authors are abstracted.

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MISCELLANEOUS COMMUNICATIONS

TO THE SECTIONS.

MATHEMATICS AND PHYSICS.

On a new case of Interference of Light. By Prof. Powell, *F.R.S.*

The author observed, that when a prism of one substance was opposed to another slightly differing in dispersive power, (as plate-glass and oil of sassafras,) so as to produce a partial achromatism, *in the coloured edges* which appeared on either side of the white image of a narrow line of light, when viewed through a small telescope, there were formed *dark bands*, about four or five being visible on each edge, parallel to the line of light.

The explanation is easy, when we consider that of the pencil of each primary ray which enters the eye, (in breadth equal to the aperture of the pupil,) the rays which have traversed greater thicknesses of the first prism, pass through less thicknesses of the second; and thus have their retardations so compensated as to be in a condition to interfere and produce the dark bands observed.

On the explanation of some Optical Phænomena observed by Sir David Brewster. By Prof. Powell, *F.R.S.*

The experiments of Sir D. Brewster here referred to were those stated at two preceding meetings of the Association, in which, on looking at the spectrum, partly through, and partly over, the edge of a plate of any transparent substance, it is seen marked by numerous dark bands. If a plate of mica or selenite be cut with an edge sloping at an angle,

compound bands are seen. In all cases the plate must advance *from* the blue end of the spectrum; the other way no bands are formed. Hence, Sir D. Brewster considers the effect due to a new and peculiar sort of polarity in the light of the spectrum.

It is allowed that the *bands simply* may be accounted for by *interference*, but *not* the *polarity*.

The author has repeated and varied these experiments; and conceives that *all* the facts are easily explicable by the principle of interference, combined with the simplest considerations relative to the undulatory course of the rays.

On fixing an opaque border, about the breadth of the pupil, on one half of the edge of the plate, while the other is left bare, bands are formed by the open part, but not by the opaque.

The inquiry was extended by using substances of different refractive and dispersive powers, both as plates and prisms, as well as plates of different thicknesses. According to these differences in the retardation, the bands were closer or wider.

The smallest *breadth* of any plate (if of the requisite thickness) will act. Hence the effect of the oblique edge is explained by a succession of edges, and was imitated artificially, by combining several plates with their edges slightly overlapping. Each plate gives its own set of bands, and thus compound systems of superposed bands are produced.

Some plates are found with natural differences of thickness at some points, on simply looking *through* which bands are seen.

These phænomena, as well as those ascribed to polarity, appear perfectly explicable on the same principle as that applied in a previous communication to the Section, viz. that the two pencils which interfere are the two halves of the pencil of each ray which converges in the eye, and whose breadth is equal to the aperture of the pupil; the intercepted half being that which has passed through the *thinner part of the prism*, and this part is the least retarded.

The intercepted part has its retardation nearly equalized with that of the other half of the pencil by the plate; while the differences in retardation for different rays of the spectrum are successively odd and even multiples of the half wave-length.

On certain points in the Wave-theory as connected with Elliptic Polarization, &c. By Prof. POWELL, *F.R.S.*

The object of this communication is to draw attention to a discrepancy between certain investigations relative to the theory of undulations, when applied to the case of elliptically polarized light, as deduced from the general equations of motion.

All the investigations of MM. Cauchy, Kelland, and others, set out from certain general equations of motion, which are then reduced into other forms, and being simplified by the *express introduction of the condition that certain terms vanish*, are shown to be directly integrable in forms which give the expression for a wave involving the relation which explains the dispersion.

If this condition (of the evanescence of certain terms,) is not admitted, the integration cannot proceed directly as in those investigations. But Mr. Tovey has shown that in this case, i. e. when those terms are finite, certain forms will still be solutions: and these are precisely those for elliptically polarized light.—(Journal of Science, No. 71.) It also appears from that paper and the author's (Phil. Trans. 1838, Part II.) that the *non-evanescence* of those terms is the *characteristic* of *elliptic* vibrations, as their *evanescence* is of *rectilinear*, and this implies the *unsymmetrical* or *symmetrical arrangement* of the molecules of æther as referred to the direction of the ray.

The discussion between Mr. Tovey and Mr. Lubbock (Journal of Science, December 1837 and January 1838,) turns upon the proposition involved in Fresnel's theory, "That every system of molecules (constituted as here supposed) has at every point three axes of elasticity: and that if these be taken as axes of co-ordinates, the *evanescence* of the terms above referred to is a necessary consequence." This proposition is essential to the whole investigation of the wave-surface; and thus it would follow, that in all media we may assume the axes so that this condition is fulfilled.

This then appears irreconcileable with the characteristic of elliptic vibrations before laid down. The author has followed up the subject in a paper which will appear in the Philosophical Transactions.

Remarks on M. Daguerre's Photogenic Process. By Fox Talbot, *Esq., F.R.S.*

The first part of M. Daguerre's process consists in exposing a silver plate to the vapour of iodine, by which it becomes covered with a stratum of iodide of silver, which is sensitive to light. Mr. Talbot stated that this fact had been known to him for some time, and that it formed the basis of one of the most curious of optical phænomena, which, as it did not appear to have been observed by M. Daguerre, he would describe to the meeting. Place a small particle of iodine, the size of a pin's head, on a plate of silver, or on a piece of silver-leaf spread on glass. Warm it very gently, and you will shortly see the particle become surrounded with a number of coloured rings, whose tints resemble those of Newton's rings. Now, if these coloured rings are brought into the light, a most singular phænomenon takes place; for the rings prove to be sensitive to the light, and their colours change, and after the lapse of a short time their original appearance is quite gone, and a new set of colours have arisen to occupy their places. These new colours are altogether unusual ones; they do not resemble anything in Newton's scale, but seem to conform to a system of their own. For instance, the first two colours are, *deep olive green*, and *deep blue inclining to black*, which is quite unlike the commencement of Newton's scale. It will be understood that the outermost ring is here accounted the first, being due to the thinnest stratum of iodide of silver, furthest from the central particle. The number of rings visible is sometimes considerable. In the centre of all, the silver-leaf becomes

white and semi-transparent, like ivory. This white spot, when heated, turns yellow, again recovering its whiteness when cold: from which it is inferred to consist of iodide of silver in a perfect state. The coloured rings seem to consist of iodide of silver in various stages of development. They have a further singular property, which, however, has not been sufficiently examined into. It is as follows: It is well known that gold-leaf is transparent, transmitting a bluish green light; but no other metal has been described as possessing coloured transparency. These rings of iodide of silver, however, possess it, being slightly transparent, and transmitting light of different colours. In order to see this, a small portion of the film should be isolated, which is best done by viewing it through a microscope. Mr. Talbot said, that he had considered the possibility of applying a silver plate thus combined with iodine to the purpose of photogenic drawing, but he had laid it aside as insufficient for that purpose, on account of its sensitiveness appearing to be much inferior to that of paper spread with chloride of silver, and therefore in an equal time it takes a much feebler impression. Now, however, M. Daguerre has disclosed the remarkable fact, that this feeble impression can be increased, brought out, and strengthened, at a subsequent time, by exposing the plate to the vapour of mercury. Another experiment was then related, in which a particle of iodine was caused to diffuse its vapour over a surface of mercury. In order to this, a copper plate was spread over with nitrate of mercury, and then rubbed very bright, and placed in a closed box along with a small cup containing iodine. The result was, a formation of Newton's rings of the greatest splendour and of a large size. But they did not appear to be in any degree sensitive to light.

The next point of M. Daguerre's process is, the exposure of the picture to the vapour of mercury; and this is by far the most enigmatical part of the whole process. For, he states that if you wish to view the picture in the usual manner, that is, vertically, you must hold the plate inclined to the vapour at an angle of 45°, and *vice versâ*. Now this is something altogether extraordinary; for who ever heard of masses of vapour possessing determinate *sides*, so as to be capable of being presented to an object at a given angle? From the hasty consideration which he had been able as yet to give to it, his first impression was, that this fact bore a certain analogy to some others which he would mention. If a piece of silver-leaf is exposed to the vapour of iodine, however uniform the tension of the vapour may be, yet it does not combine uniformly with the metal, but the combination *commences* at the edge of the leaf and spreads inwards, as is manifested by the formation of successive bands of colour parallel to the edge. This is not peculiar to silver and iodine, but occurs when other metals are exposed to other vapours: not always with entire regularity, but there is a tendency to combine in that way. A possible explanation is, that this is due to the powerful electrical effect which the sharp edges and points of bodies are known to possess; in fact, that electricity is either the cause or the attending consequence of the combination of vapour with a metallic body. Again, if a minute particle of iodine is laid on

a steel plate, it liquefies, forming an iodide of iron, and a dew spreads around the central point. Now, if this dew is examined in a good microscope, its globules are seen not to be arranged casually, but in straight lines along the edges of the minute striæ or scratches which the microscope detects even on polished surfaces. This is another proof how vapour is attracted by sharp edges, for the sides of those striæ are such. Whether or not these facts had any relation to that observed by M. Daguerre, of the action of vapour at an angle of 45°, Mr. Talbot did not pretend to say, but thought them worthy of being mentioned to the Section.

He observed, that it had been repeatedly stated in the *Comptes rendus* of the French Institute, that M. Daguerre's substance was greatly superior in sensitiveness to the English photogenic paper. It now, however, appeared that this was to be understood in a peculiar sense, inasmuch as the first or direct effect of the French method was very little apparent, and was increased by a subsequent process. This circumstance rendered it difficult to institute a direct experimental comparison between them. If it could be accomplished, he doubted whether M. Daguerre's substance would be found much more sensitive than his. The present degree of sensitiveness of the photogenic paper was stated to be as follows: it will take an impression from a common argand lamp in one minute, which is visible though weak. In ten minutes the impression is a pretty strong one. In full daylight the effect is nearly instantaneous. M. Arago had stated that M. Daguerre had obtained some indications of *colour*. Mr. Talbot thereupon referred to his paper to the Royal Society, read January 1839, and published in the *Athenæum* (No. 588), wherein he had stated the same thing. Since then, more considerable effects of colour have been noticed. In copying a coloured print the colours are visible on the photograph, especially the *red*, which is very distinct. Some descriptions of photogenic paper show this more than others; but no means have yet been found of *fixing* those colours, and sunshine reduces them all to an uniformity of mere light and shade. Sir John Herschel has formed images of the solar spectrum, in which the change of colour is seen from end to end of the spectrum, but most clearly at the red end. Mr. Talbot then mentioned a kind of photogenic pictures which afford a very capricious phænomenon. The objects are represented of a reddish colour on a white ground, and the process leaving the pictures in such a state that they are neither *fixed*, nor yet the contrary, but in an intermediate state; that is to say, that when they are exposed to sunshine they neither remain unchanged (as *fixed* pictures would do), nor are they destroyed (as *unfixed* pictures would be); but this singularity occurs, that the white ground remains unaltered, while the colour of the object delineated on it changes from reddish to black with great rapidity, after which no further change occurs. These facts (he thought) serve to illustrate the fertility of the subject, and show the great extent of yet unoccupied ground in this new branch of science.

On an Apparatus for obtaining a numerical estimate of the Intensity of Solar Light, at different periods of the day, and in different parts of the globe. By Prof. DAUBENY, *F.R.S.*

The contrivance, by which it was proposed to effect this object, was to consist of a sheet of photogenic paper, moderately sensible, rolled round a cylinder, which, by means of machinery, would uncoil at a given rate, so as to expose to the direct action of the solar rays, for the space of an hour, a strip of the whole length of the sheet, and of about an inch in diameter. Between the paper and the light was to be interposed a vessel, with plane surfaces of glass at top and bottom, and in breadth corresponding to that of the strip of paper presented. This vessel, being wedge-shaped, was fitted to contain a body of fluid of gradually increasing thickness, so that, if the latter were calculated to absorb light, the amount of it intercepted would go on augmenting from one extremity of the vessel to the other. Hence it was presumed, that any discoloration that might arise from the action of light would proceed along the surface of the paper to a greater or less extent, according as the intensity of the sun's light was such as enabled it to penetrate through a greater or lesser thickness of the fluid employed. In order to register the results, nothing more would be required than to measure, each evening, by means of a scale, how many degrees the discoloration had extended along the surface of the paper which had been exposed to light, during each successive hour of the preceding day. To render the instrument self-registering, some contrivance for placing the paper always in a similar position with reference to the sun, must of course be superadded. The object of this contrivance differed from that aimed at by Sir J. Herschel in his Actinometer, being intended as a measure of the aggregate effect of the solar intensity at the period (be it long or short) during which the paper was submitted to its influence; whereas the Actinometer merely measures the intensity at the moment the observation is made. The interposition of an absorbing fluid has at least this advantage, that it enables the observer to estimate the relative intensity, by marking the point at which the paper ceases to be discoloured, of which the eye is able to judge more exactly, than it could do, of the relative darkness of shade produced on paper which had been exposed without protection to light of different degrees of brilliancy.

Notice respecting the Use of Mica in polarizing Light. By Prof. FORBES, *F.R.S.*

The author explained the method of preparing mica used by him, since 1836, for the polarization of heat and light. The mica is exposed for a short time to an intense heat in an open fire, by which the laminæ are so subdivided, that a pellicle of extreme thinness contains a sufficient number of reflecting surfaces to polarize very completely the light or heat transmitted through it at a certain degree of obliquity. He next stated, that being struck by the resemblance to metallic lustre which the mica acquires in this process, he had examined (also in 1836)

some of its leading properties with regard to light, and he found, 1st, that the light reflected from a plate of mica so prepared, (which light is very intense,) is but feebly polarized in the plane of incidence; and 2nd, that the reflection so far resembles that at metallic surfaces, that when plane-polarized light is reflected from it, the plane of reflection being inclined to that of primitive polarization, the light is found to be elliptically polarized. The latter fact he stated to be in a great measure explained theoretically by a remark made by Professor Lloyd, to whom he had mentioned the experiment. The observation, and also the theory of it, cannot fail, he thinks, to be important in illustrating the nature of metallic reflection, which is at present so actively discussed.

On the bending of silvered Plate Glass into Mirrors. *By* JAMES NASMYTH.

The author described a simple, and, so far as he is aware, original mode of forming concave mirrors of vast size for reflecting telescopes from disks of *silvered plate glass*, which by means of the pressure of the atmosphere, he bends at pleasure in a COLD state to any required degree of curvature (within reasonable limits). Mr. Nasmyth brought to the meeting a disk of silvered plate glass, 3 ft. 3 in. diameter, which he could at pleasure bend and unbend into a concave mirror, by simply withdrawing some of the air from behind its surface; the disk of plate glass being cemented round the edge on a circular plate of cast iron, so that the air-tight cavity thus formed behind the outward surface of the glass permits the pressure of the atmosphere to act and press it into a concave mirror, the instant any portion of the air behind is withdrawn. Any required degree of curvature can be retained by simply preventing the return of the air behind by means of a stop-cock.

On Photometry, or a mode of measuring diffuse daylight comparatively, at any time and place. *By Dr.* ANDREW URE.

When lights of different intensities have the same quality of tint, or tone of colour, they may be measured relatively to each other, by the relative depths of shadow which they project upon a white wall or screen, from an opaque body interposed. When the tint of the light, however, is very different, as with the bluish flame of gas, the vivid white of an argand lamp fed with oxygen (called the *Bude* light), or the gray light of the sky, it becomes very difficult to measure the respective intensities of such lights, by comparing the shadows which they project, with the shadows projected from the flame of a standard wax candle, or a mechanical lamp. Dr. Ure experienced this difficulty, of late, upon two interesting occasions. The first was in trying to measure the relative illuminating powers of the Bude light, in subserviency to my examination before the Select Committee of the House of Commons on lighting the House. The second was in estimating

the degree of obscuration of daylight, produced by a high wooden wall or hoard, recently erected in a garden behind two valuable houses in George Street, Hanover Square, London. In the latter case he had recourse to a photometric plan, which appears to be free from all ambiguity or source of error. The chloride and several other salts of silver are so very sensitive to light, as to take a dark grey tint from exposure to diffuse daylight in a very short time. When the aspect of the sky continues uniform for two or three hours, paper imbued with the nitrate, carbonate, chloride, or phosphate of silver, will assume a depth of grey or brown tint proportional to the time of its free exposure to the day. Availing himself of this principle, Dr. Ure simultaneously placed pieces of paper so prepared, in the apartments subject to the darkening influence of the wall, and in apartments of the adjoining house, not under that influence. The papers which enjoyed the free aspect of the sky, having assumed a decided depth of hue in half an hour, he folded them up from the light, and proceeded to watch the papers placed opposite to the windows less or more obscured, till he observed them to take the same depth of tint. The relative degrees of diurnal illumination in different apartments of any house—in different countries—or on different days in the same place or country, may thus be accurately measured and permanently registered by a series of photogenic impressions of any form, which will exhibit the progressive depths of tint, after an exposure during a certain number of minutes to diffuse daylight; care being always had to prevent the direct or reflected impulsion of the sunbeams. The tints thereby produced being fixed by water of ammonia, hyposulphite of soda, or any of the well-known photogenic expedients, will serve as standards of comparison to enable us to estimate the vivacity of daylight in any region of the globe, from the time in which paper similarly prepared with a standard salt of silver, acquires from exposure to daylight the same hue. And since the comparison of tints may be made with considerable precision by an experienced eye, this photometric method may prove a valuable addition to the scientific resources of the meteorologist.

On the use of the Oxy-Hydrogen Microscope in exhibiting the phænomena of Polarization. By J. F. Goddard.

In the description of Mr. Goddard's polariscope, published in the Transactions of the Society of Arts, the polarizing mirror will be found to be placed after the third condensing lens, which is only used with the high magnifying powers; this arrangement he was compelled to adopt, as the microscope which had been previously made would not admit of any other. But he has since had one constructed, in which he could introduce every improvement that his experience suggested; and one of the most important was to place the polarizing mirror much nearer the light, so that he can now use, with the polariscope, the lowest magnifying powers, and consequently exhibit much larger illustrations and designs in selenite; also the different forms of unannealed glass; and not

only greatly to extend the variety of experiments and illustrations, but much to improve the splendour of them all. The most important of these is in the analysing part of the apparatus. Having obtained an unusually fine plate of tourmaline, he tried various experiments with it and other means, from which it appears much more can be done with a bundle of films of mica, when bleached and properly constructed for analysing, than can be effected by any tourmalines, however good. Prof. Forbes, in his experiments on the polarization of heat, first employed bundles of bleached mica,—his process of bleaching which, by heat, he has already published ; but, for experiments on light, the process must not be carried so far, and requires to be conducted with great care, for when raised beyond a bright red heat, the mica blisters and becomes unfit for these purposes, being broken into very small portions, that are incapable of transmitting a clear and distinct image, from the number of unequal refractions which the light undergoes in different parts: whilst the heat, if properly regulated, will drive off all the colour of the mica, without its being blistered; it may afterwards be easily divided into sufficiently thin laminæ, so that about eighteen of them, placed between two plates of thin glass (to protect them from being scratched), form the best means of analysing, and allowing both complementary images may be shown at the same time,—by using two screens, one to receive the refracted image, and the other to receive the reflected image,—thus furnishing the means of exhibiting, with singular effect, all the beautiful phænomena of polarized light.

A Letter to the Rev. William Whewell, President of the Section on the Chemical Action of the Solar Rays. By Sir J. F. W. HERSCHEL, *Bart., F.R.S.*

Slough, Aug. 28, 1839.

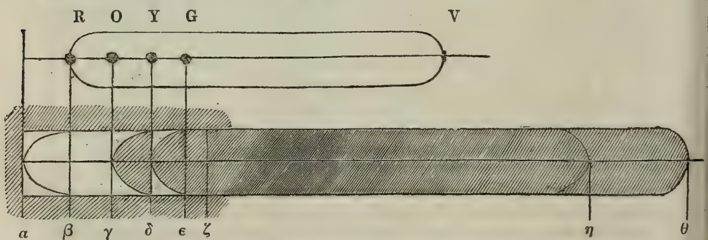
MY DEAR SIR,—May I take the liberty of requesting that you will mention to the Physical Section of the British Association a very remarkable property of the extreme red rays of the prismatic spectrum, which I have been led to notice in the prosecution of my inquiries into the action of the spectrum on paper, rendered sensitive to the chemical rays by Mr. Talbot's process, or by others of my own devising.

The property in question is this,—That the extreme red rays, (such I mean as are insulated from the red of the spectrum by a dark blue glass coloured by cobalt, and which are not *seen* in the spectrum unless the eye be defended by such a glass from the glare of the other colours,) not only have no tendency to darken the prepared paper, but actually exert a contrary influence, and *preserve* the whiteness of paper on which they are received when exposed at the same time to the action of a dispersed light sufficient of itself to produce a considerable impression. I have long suspected this to be the case, from phænomena observed in taking photographic copies of engravings; but having at length obtained demonstrative evidence of the fact, I think this may not be an improper opportunity to announce it to the President of the Physical Section of the British Association.

When a slip of sensitive paper is exposed to a highly concentrated spectrum, a picture of it is rapidly impressed on the paper, not merely in *black*, but in *colours*, a fact which I mentioned nearly two months ago, and which observation of mine seems to have been alluded to (though in terms somewhat equivocal,) by M. Arago in his account of Daguerre's process. In order to understand what follows, it will be necessary to describe the colours so depicted. The red is tolerably vivid, but is rather of a brick-colour than a pure prismatic red. And what is remarkable, its termination falls materially short of the *visible* termination of the spectrum. The green is of a sombre, metallic hue, the blue still more so, and rapidly passing into blackness. The yellow is deficient. The whole length of the chemical spectrum is not far short of double that of the luminous one, and at its more refrangible end a slight ruddy or pinkish hue begins to appear. The place of the extreme red, however, is marked by no colour, thus justifying, *so far*, the expression which M. Arago is reported to have used in speaking of my experiments—"Le rayon rouge est seul sans action."

It is impossible in this climate to form a brilliant and condensed spectrum without a good deal of dispersed light in its confines; and this light, if the exposure of the paper be prolonged, acts, of course, upon every part of its surface. The coloured picture is formed, therefore, on a ground not purely white, but rendered dusky over its whole extent, *with one remarkable exception*,—viz. in that spot where the extreme red rays fall, the whiteness of which is preserved, and becomes gradually more and more strikingly apparent, the longer the exposure and the greater the consequent general darkening of the paper.

In the figure, R V is the luminous, and $\alpha\ \theta$ the chemical spectrum. Of this the portion $\alpha\ \gamma$ is *white*, its middle corresponding to the extreme red of the luminous spectrum; $\gamma\ \delta$ is red; $\delta\ \epsilon$ green, passing rapidly through a shade of extremely sombre blue $\epsilon\ \zeta$ into black, which occupies the whole space from ζ to η.



The above is not the only singular property possessed by the extreme red rays. Their action on paper already discoloured by the other rays is still more curious and extraordinary. When the spectrum is received on paper already discoloured slightly by the violet and blue rays *only*, they produce, not a white, but a red impression, which, however, I am disposed to regard as only the commencement of a process

of discoloration, which would be complete if prolonged sufficiently. For I have found that if, instead of using a prism, a strong sunshine is transmitted through a combination of glasses carefully prepared so as to transmit absolutely no ray but that definite red at the extreme of refrangibility, a paper previously darkened by exposure under a *green* glass has its colour heightened from a sombre neutral tint to a bright red; and a specimen of paper rendered almost completely black by exposure to daylight, when exposed for some time under the same glass, assumed a rich purple hue; the rationale of which effect I am disposed to believe consists in a very slow and gradual destruction, or stripping off as it were, of layers of colour deposited or generated by the other rays, the action being quicker on the tints produced by the more refrangible rays in proportion to their refrangibilities.

It seems to me evident that a vast field is thus opened to further inquiries. A deoxidizing power has been attributed to the red rays of the spectrum, on the strength of the curious experiments of Wollaston on the discoloration of tincture of guaiacum, which ought to be repeated; but in the sensitive papers, and still more in Daguerre's marvellous ioduretted silver, we have reagents so delicate and manageable, that everything may be expected from their application.

I remain, my dear Sir,
Your very obedient servant,
J. F. W. Herschel.

P.S.—I inclose a picture of the spectrum formed as above described. It must be viewed by lamp- or candle-light—NOT BEING FIXED. In this way it may be examined by any number of persons, which by daylight would be impracticable, as a few minutes' exposure would obliterate all its peculiar characters. The larger pencil dot indicates the centre of the sun's image formed by the extreme red rays, at which point the maximum of *whiteness* will be observed to occur.

Prof. Whewell communicated some tide observations, forwarded to him by the Russian Admiral Lütke. These observations supplied—first, the tide hours of various places on the coasts of Lapland, the White Sea, the Frozen Sea, and the coasts of Nova Zemlia. These observations enable us to follow the progress of the tide-wave further than had hitherto been done. Mr. Whewell's map of Cotidal Lines (the second approximation contained in the Phil. Trans. 1836,) follows the tide only as far as the North Cape of Norway. Prof. Whewell stated, that he was informed by Admiral Lütke, that in the Frozen Sea east of Nova Zemlia there is little or no perceptible tide. The observations communicated by Admiral Lütke, offered various other results, and especially the existence of the *diurnal inequality* in the seas explored by Russian navigators, as on the coast of Kamscatcha, and the west coast of North America.

Prof. Whewell made some observations on Capt. Fitzroy's views of the tides. In the account of the voyage of H.M.SS. *Adventure* and

Beagle, just published, there is an article in the Appendix, containing remarks on the tides. Captain Fitzroy observes, that facts had led him to doubt several of the assertions made in Mr. Whewell's memoir, published in the Philosophical Transactions, 1833, and entitled 'Essay towards a First Approximation to a Map of Cotidal Lines.'*—(Appendix, p. 279.) Prof. Whewell stated, that he conceived that *doubts*, such as Captain Fitzroy's, are reasonable, till the assertions are fully substantiated by facts. Captain Fitzroy has further offered an hypothesis of the nature of the tidal motion of the waters of wide oceans, different from the hypothesis of a progressive wave, which is the basis of Prof. Whewell's researches. Captain Fitzroy conceives that in the Atlantic and the Pacific the waters oscillate laterally between the eastern and western shores of these oceans, and thus produce the tides. This supposition would explain such facts as this, that the tide takes place along the whole west coast of South America at the same time; and the supposition might be so modified as to account for the absence of tides in the central part of the ocean. Prof. Whewell stated, that he was not at all disposed to deny that such a mode of oscillation of the waters of the ocean is possible. Whether such a motion be consistent with the forces exerted by the sun and moon, is a problem of hydrodynamics hitherto unsolved, and probably very difficult. No demonstrative reason, however, has yet been published, to show, that such a motion of the ocean waters may not approach more nearly to their actual motion, than the equilibrium theory, as usually applied, does. When the actual phænomena of the tides of the Atlantic and Pacific have been fully explored, if it appear that they are of the kind supposed by Captain Fitzroy, it will be very necessary to call upon mathematicians to attempt the solution of the hydrodynamical problem, either in a rigorous or in an approximate shape.

On the best Positions of three Magnets, in reference to their mutual action. By the Rev. Professor LLOYD, *F.R.S.*

It is a problem of much importance, in connexion with the arrangement of a magnetical observatory, to determine the relative position of the magnets in such a manner, that their mutual action may be either absolutely null, or at least readily calculable. Such was stated by the author to be the object of the present investigation. In the case of *two* horizontal magnets, one of which (intended for observations of *declination*) is in the magnetic meridian, and the other (used

* Among the points which I could not establish in my own mind, by appeal to facts, were:—"The tides of the Atlantic are, at least in their main features, of a derivative kind, and are propagated from south to north;" "that the tide-wave travels from the Cape of Good Hope to the bottom of the Gulf of Guinea, in something less than four hours; that the tide-wave travels along this coast (American) from north to south, employing about twelve hours in its motion, from Acapulco to the Strait of Magalhaens;" "from the comparative narrowness of the passage, to the north (of Australia), it is almost certain that these tides must come from the southern end of the continent." "The derivative tide, which enters great oceans (North and South Pacific) from the south-east, is diffused over so wide a space that its amount is greatly reduced."

for observations of *horizontal intensity*) is in the perpendicular plane, there is nothing to compensate the action of each magnet on the other. The best thing that can be done, then, is to determine the position of the second magnet in such a manner, that the direction of its action on the first shall coincide with *the magnetic meridian*. In such case, the *mean direction* of the first magnet will be undisturbed by the second; and, as to the *variations* of the direction, it is manifest that they will be thereby increased or diminished in a *given ratio*; so that the true variations will be obtained simply by multiplying by a constant coefficient. The reciprocal action of the first magnet on the second, however, will not take place, either in the magnetic meridian, or in the plane perpendicular to it; so that the second magnet is necessarily disturbed. The case is different when a *third* magnet is introduced. When this magnet is *fixed*, we have only to consider the disturbing forces exerted upon the other two magnets, and the conditions of equilibrium of these forces are easily shown to be expressed by four equations, containing four arbitrary angles; and the equilibrium is accordingly attainable by suitably determining the position of the three magnets, whatever be their relative intensities. The third magnet may, however, be a *moveable* one, and its movements serve to exhibit the changes of one of the magnetic elements. In the Dublin Magnetical Observatory this magnet is employed in the determination of the *vertical component of the magnetic force*. It is a bar supported on knife-edges, capable of motion in a vertical plane, and brought into the horizontal position by means of a weight. The three magnets being in the same horizontal plane, it is manifest that the action of the first and second on the third must take place in that plane; and if this force be resolved into two, one in the direction of the axis of the magnet, and the other perpendicular to it, the latter component can have no effect on the position of the magnet, being at right angles to the plane in which it is constrained to move; and, as to the former, it manifestly cannot affect the mean position of the magnet, but merely augments or diminishes the deviations from that position in a given ratio. The destruction of this force, in the direction of the axis of the third magnet, introduces a *fifth* condition of equilibrium; and, as there are but four arbitrary angles, it follows that *complete* equilibrium is not attainable, except for determinate values of the relative forces of the magnets. We may, however, without inconvenience, dispense with one of the conditions of equilibrium; and, the other form being fulfilled, the disturbing action upon two of the magnets will be completely balanced, while the effect of that exerted upon the third may be at once eliminated from the results, by altering in a suitable manner the constant in the formula of reduction.

The author then proceeded to consider the cases in which the four angles were not all arbitrary, some circumstance connected with the locality determining one or more of these quantities, or establishing one or more relations among them. He pointed out, in such cases, the conditions most important to be fulfilled; and gave examples of the solution in some particular instances, such as where the three magnets are in the same right line, &c.

In reply to a question from the President, Mr. Lloyd briefly explained to the Section the arrangement of the portable observatory, adopted by Captain J. Ross, in his preparations for the Antarctic expedition. It is so constructed as to form, either *three* small separate rooms, or *one* large one; the former arrangement being desirable at places where the dip is nearly 90°, and where, consequently, the horizontal directive force is very small, and the disturbing action of the magnets on one another, relatively great. The parts are put together with copper fastenings; and the whole is so arranged, as to occupy a very small bulk when in pieces, and to be capable of being put together with quickness and security.

Meteorological Observations made at Great Malvern during the years 1835, 1836, 1837, *and* 1838. *By Mr.* ADDISON.

In these tables, the mean results for every month, and for the various seasons, and for each year, have been computed. Great Malvern, in Worcestershire, has an elevation of about 500 feet. Great differences of temperature have been observed within short distances—the lower localities being frequently very much colder than the more elevated ones. Thus on three or four occasions drops of rain have fallen, with the thermometer in the vale at 24°. The dew-point is subject to greater variations, and frequently falls to a much lower point in the higher situations, except when the temperatures are very different; it then appears, that the dew-point is frequently very low in the cold, misty, foggy air of the valley. From these tables it appears that the mean temperature of Malvern is 47·7; the highest annual mean is 49·1, in 1835; and the lowest 46·5, in 1838; being a difference of 2·5 between these two years. The mean barometer is 29·386; and the mean dew point, at 9 A.M., 43·7. When the mean temperature of the year is higher, the mean dew-point also is higher; thus, in 1835, mean temperature, 49·1—mean dew-point, 44·7; in 1838, mean temperature, 46·5—mean dew-point, 42·4. In 1837, the lowest temperature of the year occurred in the night of the 25th of March. The maximum of the barometer, in three out of the four above-mentioned years, occurred in the first week of January. The minimum of the barometer, in three out of four years, occurred in November. The range of temperature during the four years, from 9° on the 20th of January 1838, to 84° on the of 5th July 1836, is 75°. The range of the barometer, from 28·010, on the 29th of November 1838, to 30·228, on the 14th of October 1837, is 2·2 inches. The aurora borealis was observed in November, 1835; in May, 1836; in February, March, April, August, October, and November, 1837; and in September, 1838. A remarkable noise was heard at 4 P.M. on the 4th of August, 1835, like a loud clap of thunder, the air at the time being quite free from cloud, and the sun hot and brilliant. Very high winds, with the air at the dew-point, occasion a large evaporation.

On certain Meteorological Phænomena in the Ghâts of Western India. By Col. SYKES, *F.R.S.*

In the proceedings of the Physical Section at the meeting at Newcastle, the incidental mention of the annual fall of very many feet of rain in certain localities of India, instead of a few inches, as is the case in Europe, caused, I was told, some surprise, and the expression of a doubt whether the fact had been ascertained with sufficient precision, and by competent persons. I was not present on the occasion alluded to, but the doubt having been brought to my notice subsequently, I lost no time in applying to a friend to procure for me the official meteorological records kept by order of the government of Bombay at the convalescent station of Mahabuleshwar, which records I knew would afford sufficient evidence to remove all doubts, at least so far as related to one locality; and I have now the pleasure of submitting the abstract of the Meteorology for 1834; the observations being made by Dr. Murray, the medical officer in charge of that station. The station is situated lat. 17° 58′ 53″ N. and long. 73° 29′ 50″ W., near the western scarp of the Ghâts, or mountain chain extending from Surat to Cape Comorin, and varying from 1000 to 8000 feet in height. The elevation of the table-land at Mahabuleshwar averages 4500 feet. The temperature of a spring is 65·5 Fahr., and the mean temperature of the air for three or four years is nearly the same. There is a good deal of forest along the Ghâts, but in belts and patches, so that the wood can have little effect on the phænomena which I am about to describe. In this table-land is the source of the celebrated Kistnah river, which runs across the peninsula.

The following table shows the state of the thermometer, fall of rain, &c., at the station:

Months.	Mean Temperature.	Mean Maximum.	Mean Minimum.	Mean Variation.	Fall of Rain in Inches.	Direction and force of the Wind.	
						A.M.	P.M.
January ...	65·7	71·2	60·2	11·	—	E. light.	E.N.E. & W. light.
February...	67·5	73·7	61·3	12·4	0·25	E.N.E. light.	W.N.W. light.
March	74·0	81·7	66·4	15·3	—	N.E. light.	W. & N.W. fresh.
April.........	74·4	82·2	66·7	15·5	Two light showers	N.E. light.	W. light.
May	73·9	80·6	67·3	13·3	0·16	W.N.W. & S.W. fresh.	W.N.W. & S.W. fresh and strong.
June.........	66·3	69·3	63·4	5·9	32·03	S.W. high and fresh.	S.W. high and fresh.
July	64·9	66·6	63·2	3·4	118·60	S.W. strong.	S.W. strong.
August......	65·3	66·9	63·8	3·1	75·91	S.W. high.	S.W. high.
September..	65·0	66·4	63·6	2·8	65·97	S.W. fresh.	S.W. fresh.
October ...	65·5	69·4	61·7	7·7	9·29	N.E. fresh.	N.E. & S.W. light. E.N.E. fresh.
November.	63·5	69·5	57·5	12·0	—	E.N.E. high.	E.N.E. fresh.
December .	62·3	68·4	56·2	12·2	One light shower	E.N.E. fresh.	E.N.E. & W. light.
Mean...	67·3	72·1	62·6	9·5	302·21		

Hence it appears that the mean temperature of 1834 was 67·3 Fahr., that of the hottest month (April) 74·4, that of the coldest month (Dec.) 62·3; the mean maximum (April) 82·2, and the mean minimum (Dec.) 56·2. The mean variation was greatest in April, 15·5, and least in September, 2·8; and the mean variation for the year was only 9·5. The fall of rain was prodigious, being equal to 25 feet 2 inches, and this enormous mass of water fell almost entirely in the months of June, July, August and September. General Lodwick, late president at the court of Sattarah, who transmitted to me the official register, says, "I send to you a copy of Dr. Murray's meteorological table. The inches of rain are no less than 302·21. This will astonish the philosophers, but it would do more than astonish them, had they the opportunity of seeing and hearing the rain fall in torrents through a dense fog or mass of clouds which lie upon the ground for perhaps six weeks together, with a temperature by no means cold, and with little variation." The excessive fall of rain along the line of the Ghâts does not seem to be incompatible with health, for the military detachment stationed permanently at Mahabuleshwar is not characterized by any unusual sickness; and the statistical returns of the population on the hills are as healthy as those of the table-lands to the eastward. It now remains to notice some striking facts on the western side of the peninsula. The quantity of rain that falls differs exceedingly between the coast, the Ghâts within fifty miles of the coast, and the table-land eastward of the Ghâts. The mean at Bombay is 80·69. Dr. Murray shows a fall in the hills, at the elevation 4500 feet, of 302 inches, and my records at Poonah give a mean annual fall of 23·43 inches. The solution of the causes of the anomalous fall of rain does not offer any considerable difficulties. The enormous mass of vapour taken up from the Indian Ocean on approaching India, does not appear to have its upper surface at a greater elevation than five or six thousand feet, while the stratum is of great thickness; and I can bear testimony to its lower surface being below fifteen or eighteen hundred feet. The temperature of the air over the equator is necessarily very high, and its capacity for the support of aqueous vapour is proportioned to its temperature. The vapour is converted into rain, as it is driven into air of lower temperature; and, as the temperature gradually lowers proceeding to the north, and approaching the land, it follows, that out at sea, and along shore, with equal supplies of vapour, a less quantity of it would be converted into rain eastward. With respect to the prodigious fall at Mahabuleshwar and along the Ghâts, it may be accounted for by the supposition that the monsoon vapour being of low elevation and high temperature, is driven against the mural faces, and up the chasms of the Ghâts, into higher regions, and into a colder atmosphere, and is thus immediately converted into rain. The paucity of rain forty or fifty miles eastward of the Ghâts, results from the comparatively small quantity of vapour which escapes from the cold belt of air through which it is forced to pass in the hills.

An Account of some Indications of the Anemometers erected at Plymouth and Birmingham. By FOLLETT OSLER.

An anemometer and rain-gauge, similar (said Mr. Osler) to the one that I constructed about three years ago, and which has, since that period, been at work at the Birmingham Philosophical Institution, was erected at Plymouth, a few months ago, by order of the British Association, and placed under the superintendence of Mr. Snow Harris. Having just received the registers of this instrument, I shall take a cursory review of a few of them, in conjunction with those obtained at Birmingham. I shall confine my remarks entirely to the direction of the wind as registered, and not attempt on the present occasion to connect any barometric or thermometric observations with these. As it would not be possible to illustrate more than a few of these observations, I have selected those only in which the wind has been tolerably steady and strong, as being the best mode of giving a correct idea of the nature and value of these observations.

On the 17th of November, 1838, a steady wind set in at Plymouth about 8 o'clock A.M., from the S. by E., and continued until 8 o'clock on the following evening, a period of thirty-six hours. The steadiness of this current at Plymouth was very remarkable: during the first part of the time there was almost a perfect calm in Birmingham; however, by 10 o'clock in the evening, that is to say, fourteen hours after the current from the S. by E. had set in at Plymouth, a slight wind was felt at Birmingham from the north; in three hours more, that is, by 1 A.M. on the 18th of November, it became E.N.E., and finally set in a strong gale from N.E. by N., which lasted the remainder of the day. It is a singular fact, that as the gale increased in force at Birmingham it declined at Plymouth; this was towards the middle of the day; in the evening the contrary took place. The rush of air from the S. by E. at Plymouth continuing for such a length of time previous to any wind being felt at Birmingham, clearly shows that this must have been the main current; and it seems highly probable that the atmosphere for some distance north and south of this current was gradually affected, and eventually drawn into it. The state of the wind on the next day (November 19th) very much confirms this view of the subject: the current by that time became due E. at Plymouth, and N.E. at Birmingham. On the 20th there was but little wind at either place, and the directions then became the same in both places. During a considerable portion of the time much rain fell, about ·76 of an inch in Birmingham, and 1·32 at Plymouth; the two principal falls in Plymouth preceding those in Birmingham by about four hours.

On March 28 a strong wind set in from the west at Plymouth, which continued the whole day. At Birmingham the wind was S.S.W. when this gale commenced; but after continuing in that direction about twelve hours, it moved gradually round to the west, and finally to the W.N.W. During the time of this change, the strength of the wind at Plymouth increased considerably, though it did not alter

in direction. A deflection in the wind, in the opposite direction to what is now described, sometimes takes place, but not so frequently. As to whether these deviations are in *regular* curves, and are segments of large circles, or merely deflections in the course of the currents caused by some peculiarity in the situation of the places, or whether it be our insular position that modifies the currents, I cannot venture an opinion. The course of the currents is, as might have been expected, much more steady at Plymouth than at Birmingham. Thus on the 29th, 30th, and 31st of January last, the wind commenced at due west, and veered at a perfectly even rate round to the north : while in Birmingham the course of the current was exceedingly unsteady, and veered round one half the compass, in Plymouth it only moved one quarter. This, among many other instances which I could bring forward, shows that great care should be taken in the selection of stations for making observations concerning the course of the main currents of the atmosphere, which ought to be our principal object in the first instance; for we must not hope, for a long time to come, to lay down the minor fluctuations by which the greater ones are modified.

I shall conclude with a few remarks on the great storm of the 6th and 7th of January last (1839), that committed such dreadful ravages in this country, and trace its probable course and action. In addition to the records obtained by the anemometer at this place and at Plymouth, I have collected what information I could concerning the nature and extent of this storm from many parts of the British Isles. A careful analysis of these strongly leads me to the opinion that this was a small but violent rotatory storm, moving forward at the rate of about thirty to thirty-five miles per hour. The diameter of the rotating portion I am not prepared to give, nor do I consider it at all certain that it could be ascertained, as it seems likely that the revolutions were not in contact with the earth. The tendency of this eddy, or violent whirling of the air, would, of course, be to produce a vacuum in the centre. The air that forms the eddy being constantly thrown off in a slight degree spirally upwards, and dispersed in the upper portion of the atmosphere, the effect of this would be to produce a strong current upwards. Now, supposing this large eddy to be perfectly stationary, there would be a rapid rush of air towards it from all sides, which would be drawn up and thrown off through this rotating circle, and dispersed with amazing rapidity above : but as it is moving on with great velocity, the air that is in the advance of the storm is not sensibly affected until the whirl is close to it; while in the rear the motion of the air is greatly increased; first, by the tendency of the air to rush into the great vortex of the storm; and, secondly, by the motion onward of the vortex itself. This vortex or revolving column would increase in size upwards, so as somewhat to resemble a funnel; it would in fact be similar in its shape and action to an immense water-spout; whether it was vertical or not is entirely a matter of conjecture, but I should consider it probable that it would incline in the direction that the storm was moving; namely, to the N.E., and that it was an upper current that carried it in that direction. The greatest intensity of the storm in England was evidently across

Lancashire and Yorkshire. I therefore conceive that the nucleus of the hurricane passed in a N.E. direction over these two counties. Towards the sides, however, a little current set in a S. and even slightly in a S.E. direction, on the S. side of the vortex, and in a N.W. and westerly direction on the N. side, as before stated ; but the main rush was behind. The anemometer at Birmingham shows that we here first felt a fresh S. wind with a slight bearing of E. in it, which very shortly became more westerly, increasing considerably in violence, and it then moved round to the S.W., and became quite a hurricane, and continued so, very violent at first, but decreasing in strength during the remainder of the day: at Plymouth it commenced as a S.W. and then very gradually moved round a little more westward. It was by careful examination of the records of these two instruments that I arrived at the view I ventured to take of this storm; and the evidence that I have collected from various parts of the country concerning it, strongly confirms me in the opinion I have taken of it. Many violent storms followed in the wake of this extraordinary hurricane, but I have not attempted to investigate these, as the main storm must have thrown the atmosphere into so disturbed a state, that it would be very likely to produce minor eddies, gusts, &c.

On the Temperature of the Earth in the deep Mines of Lancashire and Cheshire. By Mr. EATON HODGKINSON.

These experiments were made with thermometers belonging to the Association, and in the prosecution of them the author has been very greatly assisted by the proprietors of pits and others connected with them, who have kindly undertaken to observe the results themselves—thus saving the author the trouble, in some cases, of going more than once into the mine. In the salt mines of Messrs. Worthington and Firth, at Northwich, in Cheshire, latitude about 53° 15′, a thermometer placed in a bore-hole 3 feet deep in the rock, 112 yards below the surface, indicated a temperature of 51° to $51\frac{1}{2}$° Fahr., and varied little or nothing between summer and winter. In the deep coal-mines of Messrs. Leeses, Jones, and Booth, near Oldham, a thermometer, placed in a bore-hole as before, $329\frac{1}{2}$ yards below the surface, varied from 57° to $58\frac{1}{2}$° Fahr., from observations made for a whole year by Mr. J. Swain. In the Haydock colliery, 201 yards deep, about 18 miles west of Manchester, and differing from it but little in latitude, the temperature varied considerably, both in the same hole and in different ones, but approached to 58°. The cause of these anomalies the author has not discovered. The experiments were made for him by Mr. William Fort. Other experiments are in progress. The latitude of Manchester is 53° 30′, and the mean temperature of the air there is 48° Fahr., from Dr. Dalton's experiments.

On a New Calorimeter, by which the heat disengaged in combustion may be exactly measured, with some introductory Remarks upon the Nature of different Coals. By ANDREW URE, *M.D.*

After some remarks on the quantity of sulphur in coal, and a table of results obtained by analysis, Dr. Ure thus describes his Calorimeter and its application. The apparatus which I employ consists of a large copper bath capable of holding 100 gallons of water: it is traversed, forwards and backwards, four times, in four different levels, by a zigzag horizontal flue, or flat pipe, nine inches broad, and one inch deep, ending below in a round pipe, which passes through the bottom of the copper bath, and receives there into it the top of a small black lead furnace. The interior furnace, which contains the fuel, is surrounded, at the distance of an inch, by another furnace, which case serves to prevent the dissipation of heat into the atmosphere. A pipe, from a pair of double-cylinder bellows, enters the ash-pit of the furnace at one side, and supplies a steady current of air to keep up the combustion, kindled at first by half an ounce of red-hot charcoal. So completely is the heat which is disengaged by the burning fuel absorbed by the water in the bath, that the air discharged at the top orifice has usually the same temperature as the atmosphere. In the experiments made with former water *calorimeters*, the combustion was maintained by the current of a chimney, open at bottom, which carried off at top a quantity of heat very difficult to estimate. My experiments have been directed hitherto chiefly to a comparison of the heating powers of Welsh anthracite, Llangennech, and a few other coals. I have found, that the anthracite, when burned in a peculiar way, with a certain small admixture of other coals, evolves a quantity of heat at least 35 per cent. greater than the Llangennech does, which latter is reckoned by many to be the best fuel for the purposes of steam navigation. One half-pound of anthracite, burned with my apparatus, heats 600 pounds of water 10° Fahr., viz. from 62° to 72°, the temperature of the atmosphere being 66°; whereby no fallacy is occasioned either by the conducting powers of the surrounding medium, or by a chimney current. We thus see that one pound of anthracite will communicate, to at least 12,000 times its weight of water, an elevation of temperature of 1°, by Fahrenheit's scale. For the sake of brevity, we may call this quantity, or energy, 12,000 unities of heat. One pound of Llangennech, in the same circumstances, will afford 9,000 unities: one pound of good charcoal, after ordinary exposure to the air, affords 10,500: perfectly anhydrous charcoal would yield much more: one pound of Lambton's Wall's-end coals affords 7,500 unities. It deserves to be remarked, that a coal, which produces in its ignition much carburetted hydrogen and water, does not afford so much heat as a coal equally rich in carbon, but of a less hydrogenated nature, because, towards the production of the carburetted hydrogen and water a great deal of latent or specific heat is required: indeed, the evaporation of unburnt volatile matter from ordinary flaming coals abstracts unprofitably a very large portion

of their heat, which they would otherwise afford. Hence, those chemists who, with M. Berthier and Mr. Richardson, estimate the calorific powers of coals by the quantity of carbon which they contain, or the quantity of oxygen which they consume, have arrived at very erroneous conclusions. The amount of error may be detected by experiments on the cokes of flaming coals. M. Berthier examines coals for their proportion of carbon, by igniting a mixture of each, finely pulverized, with litharge, in a crucible, and estimates 1 part of carbon for every 34 parts of lead which is reduced. I have made many researches in this way with both charcoal and anthracite, and have obtained very discordant results. In one experiment, 10 grains of pulverized anthracite, from the vale of Swansea, mixed with 500 grains of pure litharge, afforded 380 grains of metallic lead; in a second similar experiment, 10 grains of the very same anthracite afforded 450 grains of lead; in a third, 350 grains. In one experiment with good ordinary charcoal, fresh calcined, 10 grains, mixed with 1,000 of litharge, afforded no less than 603 grains of metal. The crucible was in each case covered and luted. My future researches, which are intended to embrace every important variety of fuel, natural and artificial, will be made with an apparatus somewhat modified from that here described. Three furnaces will be inclosed within each other, with a stratum of air or ground charcoal between each, so as to prevent all loss of heat into the atmosphere, and thereby to transfer the whole heat disengaged by combustion into a large body of water, of a temperature so much below that of the atmosphere at the beginning of the experiment, as it shall be above it at the conclusion.

On a method of filling a Barometer without the aid of an Air-pump, and of obtaining an invariable level of the surface of the Mercury in the Cistern. By Prof. STEVELLY.

Prof. Stevelly said that it was very difficult to fill a barometer tube so as to be quite free from air and moisture. Mr. Daniell, in his Meteorological Essays, proposed to fill the barometer under the exhausted receiver of the air-pump, and actually had the barometer of the Royal Society so filled by Mr. Newman, under his own superintendence; but, although an expert London working optician might be found capable of executing successfully such a task, yet few in the country could hope for such an advantage; and, in fact, although he had attempted the process at Belfast, he had never succeeded. After some consideration, the following simple mode of using the Torricellian vacuum of the tube itself, instead of the air-pump, in filling it, occurred to him. He heated the mercury as hot as it could be handled, and filled the tube, in the common way, to within half an inch of the top; then worked out, in the usual way, all air-bubbles, as perfectly as possible; filled up the tube to the top, and inverted it in a cup of hot mercury, when of course it subsided, in the upper part of the tube, to the barometric height; he then placed his finger on the mouth of the tube, under the mercury in the

cup, and lifted it out; and, still holding his finger tightly over the mouth of the tube, laid it flat on a table, when the mercury in the tube soon lay at the under side of the tube, leaving the upper part along the length of the tube void. Upon then turning the tube slowly round, still keeping the finger on its mouth, every speck of air was gathered up. He then placed the tube in an upright position, with its mouth upwards, still keeping the finger firmly on; and, placing a funnel of clean dry paper about the upper part, an assistant filled the funnel with hot mercury, so as to cover the finger. Upon slowly withdrawing the finger, the mercury went gently in, and displaced almost perfectly the atmospheric air which had gathered into the void space. By renewing the process which succeeded the previous washing of the air out of the tube, once, or at most twice, a column of the most perfect brilliancy was obtained. He had mentioned this simple method to Dr. Robinson, of Armagh, who suggested that, to get rid of the damp and greasiness of the finger, it would be better to cover it during the process with clean and dry caoutchouc; and this was found a decided advantage.

The method of procuring an invariable surface in the cistern was equally simple. From the imperfection of the author's sight, it was an object of much interest to him to have as few readings or adjustments depending on sight as possible. He proposed, therefore, to divide the cistern into two compartments, by a diaphragm of sheet iron or glass, brought to a sharp edge at top. Into one of these compartments the barometer tube dips; in the other is placed a plunger of glass or cast iron, which can be raised or lowered by a slow screw movement. To prepare for an observation, the plunger is first screwed down, by which it displaces the mercury in one compartment, and raises its surface in the other above the edge of the diaphragm; upon raising it slowly again, the mercury drains off to the level of the edge of the diaphragm, thus, at every observation, reducing the surface to a fixed level.

A letter was received from Prof. A. D. Bache of Philadelphia, on the subject of rain at different heights. It is expected that this and other subjects will be treated of in the Report on the Meteorology of the United States of America, which Mr. Bache has undertaken to draw up for the Association.

Experiments to determine the Fluency or Viscidity of different Liquids at the same Temperature, and of the same Liquids at different Temperatures. By Dr. Ure.

The author, referring to a memoir read to the Society of Civil Engineers, states a new mode of experiment and gives the results as under.

Upon this occasion I put the liquid, either cold or heated to a certain temperature, into a glass funnel, terminated at its beak with a glass tube of uniform bore, about one eighth of an inch in diameter, and three inches long. The funnel was supported in a chemical stand, and dis-

charged its contents, on withdrawing a wooden pin from the beak, into a glass goblet placed beneath, alongside of which a chronometer was placed to indicate, in seconds, the time of efflux. The volume of liquid used in each case was the same,—viz. 2,000 grain-measures, at 65° Fahr. The times of efflux with liquids of the same specific gravity and bulk, in the same vessel, vary with the viscidity of the liquids, and serve to measure it. A correction ought to be introduced in estimating the times of efflux of hot liquids, on account of the enlargement, by expansion, of the bore of the glass tube; but this, being a point of little consequence in the practical application of this inquiry, has been neglected.

2,000 grain-measures of water, at	60° Fahr., ran off in	14 sec.
	68	13
	164	12.

When the funnel and glass tube were faintly smeared with oil, though perfectly pervious, and apparently clean, boiling hot water having been run through them,

2,000 grain-measures of water, at	150° Fahr., ran off in	24 sec.
	142	23
	94	24
	56	25.

So great is the repulsive influence between oil and water, in retarding the flow of the latter through a small orifice.

2,000 grain-measures of	Fahr.	Spec. grav.	Sec.
Oil of turpentine	65°	0·874	14
Pyroxylic spirit	„	0·830	14½
Alcohol	„	0·830	16
Nitric acid	„	1·340	13½
Sulphuric acid	„	1·840	21
Ditto	262		15
Saturated solution of sea salt	65	1·200	13
Sperm oil	„	0·890	45½
Fine rape-seed oil	„	0·920	100
Fine pale seal oil	„	0·925	66
Fine South Sea whale oil	„	0·920	66
Sperm oil	254		15
Rape-seed oil	254		17
South Sea oil	250		17

The rape-seed oil is so viscid, as to burn with difficulty in lamps of the ordinary construction, but in the hot oil lamp of Parker it affords a very vivid light. In my former apparatus, the difference of level between the two legs of the siphon, which constituted the effective pressure of efflux, was only half an inch, whereby 2,000 grain-measures of sperm oil, at 64°, took no less than 2,700 seconds to run off, while that volume of oil of turpentine ran off in 95 seconds. It would therefore appear that the fluency of a viscid oil diminishes in a very rapid ratio with the diminution of pressure. Hence, an oil will burn well in a mechanical lamp, where it is raised to the level of the bottom of the

flame by pump work, which will afford a very indifferent light in one of the French Annular or Sinumbral lamps, where the supply is given by a very slight pressure.

Notice of a comparative Pendulum. By W. J. FRODSHAM, *F.R.S.*

The principle of this pendulum is the same as that which was proposed some years ago by Mr. Reid of Woolwich, but the construction is different. Over the steel rod of a common pendulum Mr. Frodsham slips a zinc tube, which rests on the adjusting screw at the lower end of the rod, the bob being fastened at the centre by two connecting rods of steel to the tube at the point where the expansion of the tube is the same as that of the rod. By making the zinc tube a little too short, and applying small rings cut from the same tube, as correcting pieces, until the proper length is found, the evils of the irregularity of expansion of different specimens of the same metals are overcome. The author describes the means whereby he secures access for the air to the zinc tube where it passes through the bob, and to its inner surface surrounding the steel rod.

Mr. Frodsham also describes improvements in the mode of suspending the pendulum, particularly the application of a brass tube, called an 'isochronal piece,' which slides on the rod, and at its upper part is made to embrace the suspending spring, the acting part of which is thus made variable in length without affecting the compensation of the pendulum. When the suspending spring is in a state of rest and in its natural and unconstrained position, the isochronal piece is made to embrace and unite it firmly to the rod, by two screws at the upper end, thereby preventing any strain or warp to which the spring is subject by the method usually employed. To any given weight of the bob of the pendulum, it appears that some particular length and strength of the suspending spring is better adapted than any other to produce isochronism in the pendulum, and the use of the 'isochronal piece,' in producing in any spring the nearest approach to this condition, is obvious.

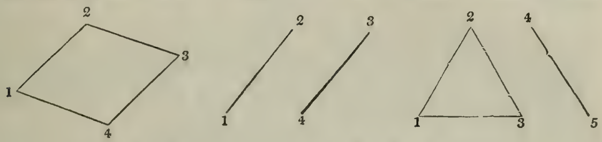
On the Motion of Points or Atoms subject to any law of force. By J. K. SMYTHIES.

The method of investigating the motions of points proposed is, to find equations necessarily existing between their distances; thence to deduce others involving any required combination of the distances, and their differentials of any orders necessarily true for all moving systems; and then, combining those equations, which assign a particular law of motion, with those which are true for all motions, to eliminate the differentials of all or any required number of orders by a simple mode of elimination.

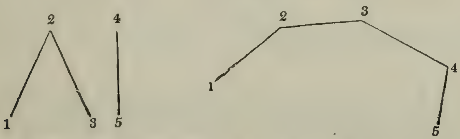
If there are any number of points (n) in space, the following equation subsists between their distances, where $\overline{12}$ or $\overline{21}$ denotes the

distance between the 1st and 2nd points, &c.; and a term with Σ prefixed denotes the sum of all the terms of the same type, terms being of the same type when deducible from each other by substituting one figure for another, or by any number of successive substitutions.

$$2(n-4)\,\Sigma\,\overline{12}^2.\overline{23}^2.\overline{34}^2.\overline{41}^2 \quad -(n-4)\,\Sigma\,\overline{12}^4.\overline{34}^4 \quad +4\,\Sigma\,\overline{12}^2.\overline{23}^2.\overline{31}^2.\overline{45}^2$$



$$+\,2\,\Sigma\,\overline{12}^2.\overline{23}^2.\overline{45}^4 \qquad -\,2\,\Sigma\,\overline{12}^2.\overline{23}^2.\overline{34}^2.\overline{45}^2 = 0.$$



The subscribed diagram shows how the lines are connected in each type.

If for each line we substitute its differential of the mth order, the resulting equation is true: and generally, if we substitute for each line the line increased by the sum of its differentials of every order up to the mth, and separate all the terms involving products of the same given dimensions for every order of differentials, their sum equals zero. Representing a function of the distances and their differentials symmetrical for the 1st and 2nd points, and also for the rest, thus F $(\overline{12}\ \overline{34\ldots n})$ all these equations can be put under the form $\Sigma d^m\, 12.\mathrm{F}\,(\overline{12}\ \overline{34\ldots n}) = 0$.

These equations will be sufficient when the law of motion of the system is assigned by equations involving only the distances and their differentials; but when it involves the absolute velocities of the points along their own paths, we must find general equations between the distances, the relative velocities of the points to or from each other represented by the first differentials of the distances, and their absolute velocities along their own paths. For this purpose divide $2\,n$ points into two equal groups, denoting two successive simultaneous positions of the n points. Draw right lines from each point to every other in its own group, and to one in the other. An equation may be found between these n^2 lines thus drawn between the $2\,n$ points. When the n lines connecting the two groups are indefinitely diminished, their limiting values denote the absolute velocities of the points; and the limiting differences of the distances of the same two bodies, in the two groups, denote their relative velocities, or the first differentials of the distances. Equations involving higher orders of differentials may be deduced from this as from the preceding.

By combining these equations with those which assign the law of motion, it is always possible to eliminate all the differentials if the number of points (n) be taken great enough; and when we assume (as in all physical applications to general laws of force we must,) that the distance between every two points is the same function of the masses, positions and motions of the rest, it may be effected in a simple manner.

On certain Results, regarding the minimum thickness of the Crust of the Globe, which might be consistent with the observed phænomena of Precession and Nutation, assuming the earth to have been originally fluid. By WILLIAM HOPKINS, *M.A., F.R.S., &c.*

The mathematical investigation is intended to appear in the Transactions of the Royal Society.

Notice of certain Analytic Theorems. By CHARLES BLACKBURN, *M.A.*

Remarks on Dr. Wollaston's argument respecting the infinite Divisibility of Matter, drawn from the finite Extent of the Atmosphere. By the Rev. WM. WHEWELL, *F.R.S.*

He observed, that Dr. Wollaston had proceeded on this supposition: That if the extent of the earth's atmosphere be finite, air must consist of indivisible atoms; since, as Dr. Wollaston assumed, the only way in which we can conceive an upper surface of the atmosphere is, by conceiving an upper stratum of atoms, the weight of which, acting downwards, is balanced by the repulsive force of the inferior strata acting upwards. Mr. Whewell maintained, that such a mode of conception was altogether arbitrary, and the argument founded upon it quite baseless; for if we investigate the relation between the height of any point in the atmosphere, and the density of the air at that point, upon the supposition that the compressing force is as the nth power of the density, we find that the density vanishes at a finite height whenever n is greater than unity. Therefore, though the atmosphere do not consist of indivisible particles, it will still have a finite surface. In fact, the finite surface of the atmosphere no more proves the atomic constitution of air, than the finite surface of water, in a vessel, proves the atomic constitution of water. But it will still be asked, how then is the highest stratum of air supported? To which the answer is, that there is no highest stratum of definite thickness. Supposing the atmosphere finite, every upper stratum of air bounded by the upper surface of the atmosphere, has the upper part of that stratum supported by the lower; and however thin the stratum be, it has still an upper and a lower part which have this relation to each other. The question, What supports the *uppermost* stratum of the atmosphere? is of the same kind as the question formerly discussed by writers on mechanics, What is the velocity with which a body *begins* to fall?

Account of a recent successful Experiment to determine, by means of Chronometers, the difference of Longitude between Greenwich and New York. By E. J. Dent.

The rapid transmission of chronometers now practicable by means of steam-vessels from one meridian to another, offers great facilities for the determination of the differences of longitude. This led me (said Mr. Dent) to embark four chronometers on board the *British Queen* steam-vessel, on her first voyage from England to America. Captain Roberts, the commander of the vessel, kindly undertook the charge of them, and (through the interest of Messrs. E. and G. W. Blunt, of New York), Jesse Hoyt, Esq., the collector of customs at that port, gave a free permit, as well as every other facility, for landing them. They were then compared daily with two astronomical clocks at the observatory at Brooklyn, 4,700 feet, or 4·09 sec., east of the City Hall, in New York. The errors of these clocks were determined by transit-observations on the days of arrival and departure. The errors of the clock with which the chronometers were compared at Greenwich immediately before the embarkation on board the *British Queen*, and also immediately after their landing at Greenwich from that vessel on their return, were determined by means of several series of zenith-distances of stars on both sides of the meridian, and also of the sun, taken with a sixteen-inch altitude and azimuth instrument at the Royal Naval Schools, Greenwich, by the Rev. George Fisher. The stone pedestal, on which this instrument was placed, is, by actual measurement, 560 feet, or $0^{s}{\cdot}6$ west of the transit-instrument at the Royal Observatory, which quantity is, of course, applied to determine the Greenwich error. In determining the difference of longitude in the present case, I use the methods which I employed, first, in my journey for the same object between Greenwich and Paris, and subsequently, in the other experiments which I have made to determine the difference of longitude between Greenwich and Oxford, Dublin, Armagh, Edinburgh, Cambridge, &c. The first method is by means of the *travelling rate*, the second is by the *stationary rate**. The "travelling rate" is the mean rate during the voyage, obtained by dividing the difference between the previous and subsequent errors at Greenwich, by the number of days absent. The "stationary rate" is a mean of the rates determined, 1st, at Greenwich, before the chronometers were embarked; 2nd, at Brooklyn, after their disembarkation there; and 3rd, at Greenwich, on their return, after their landing. The first method is, no doubt, the more unexceptionable of the two; it involves, indeed, the supposition of the *outward-bound rates* being the *same* as the *homeward-bound* ones; yet as errors, arising from the magnetic action of the iron in the vessel upon the chronometers, or other causes, would, in all probability, be in excess and defect to the same amount, we might therefore reasonably expect a compensation of errors to occur, or nearly so. It is very remarkable, that on board the steam-vessel in *all* the chronometers, the *mean* "*tra-*

* These have been sometimes called the "shore rates" and the "ship rates."

velling rate" differs from the *mean* "*stationary rate*" *in the same way*, or the *losing* rates were increased, and the *gaining* ones diminished. Whether, however, we use the travelling rate or the stationary rate for the determination of the difference of longitude of the two places, we we obtain results extremely near to each other, provided we take the means between the *outward-* and the *homeward-bound* determinations. This was shown by calculations submitted to the Section. Taking the first result, or that given by the *travelling rate*, as the true difference of longitude between the Observatories of Brooklyn and Greenwich, and applying the quantity, 4·09 sec. for the difference of meridians between the Observatory of Brooklyn and the City Hall, in New York, we have for the difference of longitude between the latter place and Greenwich $4^h\ 56^m\ 7^s{\cdot}08$ west. The longitude of New York from Paris, as given in the *Connaissance des Tems*, by M. Daussy, is $5^h\ 5^m\ 22^s{\cdot}0$: if from this be deducted $9^m\ 21^s{\cdot}28$, the difference of longitude between Greenwich and Paris, as determined by the chronometrical experiments made by me between those two places in 1837, we shall have $4^h\ 56^m\ 0^s.72$ as the difference of longitude, according to that observer, between Greenwich and New York.

Comparison of Results.

	h.	m.	s.
First, by the chronometers, the longitude of New York is west from Greenwich	4	56	7·08
Second, by M. Daussy, as given in the *Connaissance des Tems*	4	56	0·72
Difference			6·36

This difference is little more than one half of a mile in longitude; and the smallness of it proves that this, (*the first result by the transit of chronometers from England to America,*) removes the apprehensions which have been suggested that chronometers may not go well in steam-vessels.

Note, by Mr. Dent, *accompanying a Table of the Rate of the Transit-Clock in the Radclyff Observatory, Oxford, was then read.*

In 1838 a transit-clock was made by Messrs. Arnold and Dent for the Radclyff Observatory, Oxford, to which, by the special desire of the late Professor Rigaud, was attached the improved mercurial pendulum with its cistern of cast iron, &c. In the statement now submitted of the going of this clock will be found, said Mr. Dent, a mean daily rate, which, when corrected for an *intentional* over-compensation, has been rarely equalled; the amount of the correction for this over-compensation is, I think for many reasons, a subject for experiment alone: I conceive it cannot be calculated but with extreme difficulty—1st, because the centre of oscillation must be disturbed, and then an additional correction becomes necessary, in consequence of the alteration in the pendulum-rod to bring it to time; 2nd, because some quantity is due to the

change of elasticity produced by extremes of temperature in the suspension-spring; and 3rd, because another quantity is due to that effect of extreme cold which is indicated by a decrease in the vibration-arc of the pendulum. In the present case, the reduction of compensation required is so small, that until some cause for stopping the clock occurs, Professor Johnson is not anxious that the compensation shall be attempted.

The following is the monthly abstract of the mean daily rate of the transit-clock, as observed by the late Savilian Professor of Astronomy:

	Mean Daily Rate.	Therm.
	sec.	°
1838, October	−0·136	50·14
„ November	−0·511	44·18
„ December	−0·987	41·47
1839, January	−0·887	38·45
„ February	−0·544	42·29
„ March	−0·414	41·18
„ April	−0·060	55·57
„ May	+0·016	55·38
„ June	+0·222	62·57
„ July	+0·375	65·00
„ August	+0·223	64·53

On Natural Perspective. By Mr. Parsey.

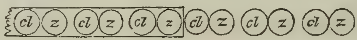
On an analogy between the atomic weights of certain Gases and the expansions of the primitive colours of the Solar Spectrum. By Lieut. Morrison, *R.N.*

CHEMISTRY.

On the Theory of the Voltaic Circle. By Prof. Graham.

Professor Graham explained the views now received of the propagation of electrical induction through the fluid and solid elements of the voltaic circle, by the formation of chains of polar molecules, each of which has a positive and negative side, and in which no circulation of the electricities is supposed, but merely their displacement and separation from each other in each polar molecule. These electricities

in the polar molecule of hydrochloric acid, for instance, are displaced, when the acid acts as an exciting fluid, and the positive electricity is located in the chlorine atom, and the negative electricity in the hydrogen atom. The electricities are, at the same time, made the depositories of the chemical affinities of the chlorine and hydrogen respectively. Mr. Graham proposed to modify this hypothesis so far as to abandon the idea of electricities being actually possessed by these bodies, and to refer the phenomena at once to the proper chemical affinities of the bodies. He assigned similarly polar molecules to the exciting fluid and metals; and taking hydrochloric acid as a type of exciting fluids, he gave to each molecule a pole, having an affinity resembling that of chlorine, or *chlorous* affinity, instead of negative electricity, and another pole, having an affinity resembling that of zinc or hydrogen, or *zincous* affinity, instead of positive electricity. When zinc and acid are in contact, the polar state of a single chain of molecules might be represented as in the figure.



The particle of acid B, next the zinc, has its chlorine atom in contact with the metal and its hydrogen atom distant from it, marked respectively *cl* and *z* in the figure. Part of the affinity of *cl* being engaged by the zinc, the hydrogen is so far received from that affinity, and thus attracts the *cl* of C. Thus, by a sort of induction, the *z* of B causes the *cl* of C to be chlorous, or the molecule of acid C to become polar, and that again the molecule D. In the zinc, (the molecule being supposed to contain two chemical atoms,) while the external atom of A becomes zincous, from its contact with the acid, the other atom becomes chlorous; so that these atoms of this molecule may be marked *cl* and *z*, and so also the molecules E and I of the zinc, which become polar by induction.

In another diagram Professor Graham showed how this chemico-polar condition is propagated round a voltaic circle. The molecules of the zinc and acid being polar by contact, which is sufficient to develop their affinities, an induction one way through the zinc, and in the opposite direction through the acid, conspire to produce the same polar condition in the molecules. The result is, that the molecule *z* of A is zincous both primarily and by induction, and its affinity for the atom *cl* of B greatly increased; and, consequently, combination can take place between these atoms when the circuit is completed, but not otherwise.

If the connecting wire be broken, and a decomposable liquid, such as hydriodic acid, be interposed between the extremities, a chain of polar molecules comes also to be established in that liquid, the iodine (which is the analogue of chlorine) being the seat of the chlorous affinity, and the hydrogen the seat of the zincous affinity. The extremity of the wire connected with the copper plate is zincous, or has zincous affinity,

and consequently attracts the iodine which appears there, when decomposition occurs. The extremity of the wire connected with the copper plate is chlorous, or has the affinity of chlorine; and consequently, the hydrogen of the hydriodic acid is eliminated there when decomposition occurs. These poles in the decomposing cell of the voltaic circle have, from their importance, always received peculiar appellations, which, with two other terms, Mr. Graham changes as follows:

Chlorous = Negative.
Zincous = Positive.
Chloroid = The negative pole, the cathode, the platinode.
Zincoid = The positive pole, the anode, the zincode.

Mr. Graham afterwards endeavoured to show, that electrolytes were bodies which, like hydrochloric acid, possessed a salt radical and basyle element, which might be the seat of the chlorous and zincous affinities, and which might, indeed, be called the chlorous and zincous elements of the electrolyte; so that the same view was applicable to electrolytes in general*.

Notice of new Electro-chemical Researches. By Professor Schönbein, *(of Basle).*

"The discovery of the chemical power of the voltaic pile made in the beginning of our century by British philosophers, could not fail drawing the attention of the scientific world upon the relations which exist between chemical and electrical phænomena. Indeed, only a few years after this important fact had been acertained, your illustrious countryman, Sir Humphry Davy, as well as the celebrated Swedish philosopher Berzelius, did not hesitate to establish the theory which has since been generally adopted, and which is founded upon the principle that chemical and electrical forces are essentially the same.

"Having almost exclusively occupied myself these last six years with researches bearing upon the subject in question, and having obtained from them some results which seem to be altogether irreconcileable with the very first principles of the electro-chemical theory, I entertain the hopes, that by making known the details of the investigations alluded to, I shall render some service to science."

From a review of the consequences flowing from the ordinary electro-chemical theory, M. Schönbein observes:

"It follows from the doctrines laid down by Davy and Berzelius, that any metal put by any means into the negatively electrical state, has its affinity for oxygen either diminished or altogether destroyed, so as to cease to be an oxidable metal under ordinary circumstances. Now let us see how far facts agree with the principles of the electro-chemical theory.

* Mr. Graham has since developed his views more fully in the Third Part of his Elements of Chemistry, pp. 197—241.

First fact.

A piece of iron was voltaically associated with a piece of zinc, and each of these metals put into a separate vessel filled with common water. The vessels did not communicate with each other. Only a few hours after the immersion of the iron had been effected, light flakes of oxide of iron made their appearance round the metal; and, after a couple of days, the latter was corroded to a considerable degree. The same result was obtained when I plunged the iron piece into water, and made the zinc rise above the level of the fluid, so as to prevent the latter metal from being in the least contact with water. According to the judgement of my eye, a piece of iron, being immersed into water without any voltaic association, was no more corroded than that metal appeared to be under the circumstances just stated.

Second fact.

Two pieces of iron wire were made, one of them the positive, the other the negative pole of a voltaic pile, which consisted of ten pairs of copper and zinc, and was charged with water holding 5 per cent. of common salt dissolved. Each of the polar wires was put into a separate vessel filled with common water, so as to leave the pile unclosed. Under these circumstances, both wires were equally attacked and corroded, in the same manner as if a single piece of iron had been put into water; for, after the lapse of a couple of hours, the polar wires were seen to be surrounded by light flakes of oxide of iron.

Third fact.

A piece of iron being voltaically associated with zinc, was exposed to the action of the atmosphere. Having left this voltaic pair for some time to itself, the iron part of it appeared to be covered with a thin layer of rust; and on comparing it with a piece of iron which had also been placed within the atmosphere during the same space of time, I could not see any notable difference between the states of the surfaces of both pieces.

Fourth fact.

A piece of iron wire was connected with each of the poles of a voltaic pile, without making the wires touch each other. Being exposed to the action of the atmosphere, under these circumstances, both polar wires appeared, after some time, equally affected by rust, and just so as another piece of iron did which was not connected with a pile.

Fifth fact.

A piece of iron, being voltaically associated with zinc, was put into common water, so that both metals took up their place within the same vessel. Though I have kept that voltaic pair within water these last twelve months, the iron part of it does not appear to be in the least oxidized, its surface being perfectly brilliant.

Sixth fact.

A piece of iron wire was connected with each of the poles of a pile, and each of these pieces made to plunge into a separate vessel filled with common water, both vessels being connected by the means of a piece of platina. That part of the negative polar wire which was immersed in water did not rust at all, as long as there was a current passing through the arrangement.

Seventh fact.

Copper being intimately associated with zinc, and brought into an aqueous solution of chloride of sodium in such a manner that each of the metals did plunge into a separate vessel, was soon chemically affected; provided, however, both vessels were not communicating with each other.

Eighth fact.

The same experiment was made as in the preceding case, with the difference however that both metals did plunge into the same vessel. Under these circumstances the copper piece was not in the least corroded by the salt water, whatever was the length of time during which I kept the metals immersed.

Ninth fact.

A piece of copper was connected with each of the poles of a voltaic pile, and put into a vessel holding an aqueous solution of common salt. Both pieces were attacked by the fluid, just in the same way as if they had not been attached to a voltaic arrangement, provided both vessels did not communicate with each other.

Tenth fact.

The experiment was made as in the preceding case, with the difference only that both vessels were caused to communicate with each other by the means of a piece of platina. The positive polar wire quickly underwent oxidation, whilst the negative one remained untouched. If an aqueous solution of common salt was made use of as the exciting fluid in the pile, and the latter left unclosed, the copper pieces of the voltaic pairs rather readily entered into oxidation, whilst they were not at all chemically affected when the pile was closed.

Eleventh fact.

A piece either of copper or of iron was connected with each of the poles of a pile; two tumblers were filled, partly with mercury, partly with water, or with a solution of common salt, and both vessels made to communicate with each other by a piece of platina, so as to make each extremity of the latter enter into the mercury of either vessel. Things having been arranged in the manner described, the polar wires were introduced, each of them into one of the tumblers, so that the free end of each wire was made to plunge into the mercury. Under

these circumstances both polar wires appeared to be equally affected by the non-metallic fluids, i. e. so as they would have been if not connected with any voltaic arrangement."

On the basis of these facts, corroborated by various collateral phænomena, M. Schönbein ventures to assert, that in the common case, when copper or iron, acting as a negative electrode, in an aqueous fluid holding oxygen dissolved, is not chemically affected by the latter element, voltaic action has directly nothing to do with the protection of the iron or copper. He then proceeds to explain his theory of the galvanization of metals, by applying it to this particular case, and finally affirms the following propositions:

1. Neither common nor voltaic electricity is capable of changing the chemical bearings of any body; and the principles of the electro-chemical theory, as laid down by Davy and Berzelius, are fallacious.

2. The change which certain metallic bodies, being placed under the influence of a current, seem to undergo with regard to their chemical relations, is due to some substance or other being produced and deposited upon those bodies by the agency of current electricity.

3. The condition, *sine qua non*, for efficaciously protecting readily oxidable metals against the action of free oxygen being dissolved in fluids, is to arrange a closed voltaic circle, being made up on one side of the metal to be protected, and another metallic body more readily oxidable than the former, and on the other side of an electrolyte containing hydrogen; for instance water.

Researches on the Electrical Currents on Metalliferous Veins made in the mine Himmelsfurst, near Freyberg. By Prof. Reich.

Since Mr. Fox first discovered the fact in copper mines in Cornwall, it has been known, that an electrical current is indicated by Schweigger's multiplier, when two points, where ore presents itself, are connected by a metallic wire, whether these be in the same or different veins. Mr. Fox repeated the experiments in lead veins, with similar results. On the other hand, Von Strombeck (Karsten's *Archiv*, vi. p. 431) could find no trace of such electrical currents in lead and copper veins on the right bank of the Rhine; again, Henwood repeated the experiments in Cornwall, and confirmed the results of Fox. Prof. Reich has made similar experiments in the mine Himmelsfurst, which lead to very decisive fundamental results. The method of experiment was in the main that of Fox. When the two points to be connected were determined, a fresh surface was first worked on each, and on this a disc of copper 6 inches long, 3 inches wide, was kept firmly pressed by a piece of wood. An uncovered end of a copper wire, spun over with silk, was kept pressed to the copper plate by means of a clamp. The one wire was always short; the other, about 180 metres long, was rolled on a reel. This latter was retained in all the experiments, the current having thus the same length of wire to pass, so that its influence on the amount of deviation of the needle was constant. The long wire

was let out till it reached to the second point of contact, near which the multiplier was placed, and the two ends of the fine wires connected with it. The multiplier, with double needle, and very sensible, belonged to a thermo-electric apparatus of Melloni, made by Oertling of Berlin. In order in some measure to judge of its delicacy, it may be mentioned, that a current, from a pair of zinc and copper plates of only one inch square, placed in water very weakly acidulated, drove the needle up to the button, placed at 90°, to prevent further deviation; that an iron wire connected with two brass wires, placed in the multiplier, by the mere heating with the hand of the point of contact, produced a deviation of from 10° to 20°, according to the temperature communicated. The following results were obtained:—1. Two ore points, separated by a non-metalliferous mass, or between which there occurs a cross vein, or a space where the vein is worked out, give rise to an electric current in a metallic wire connecting them. This law was determined by seventeen experiments with every variety in them, so as to obviate all objections. 2. Two ore points, in uninterrupted metallic connection with one another, induce no electrical current through a wire connecting them. 3. If only one disc be connected with an ore point, and the other with the timbering, or be held in the hand, there is no effect produced on the multiplier. This result was confirmed several times. 4. If an ore point be connected with masses of ore already won, a current sometimes manifests itself, and sometimes there is none. 5. When an ore point is connected with non-metalliferous rock, frequently no current takes place; frequently, however, a current, always feeble, occurs in the connecting wire. This result does not agree with that of Fox and Henwood, who never detected a current. Professor Reich performed the experiment eighteen times.

With respect to the cause of the electrical currents, observed in metalliferous veins, three different opinions have been broached. They have been ascribed, 1, to general electric currents at the earth's surface, produced either entirely or in part by the earth's magnetism: 2, to hydro-electric, and 3, to thermo-electric actions of the various metallic components of the vein. The first hypothesis, according to Reich, is refuted by the independence of the direction of the currents on their position relatively to the earth's axis. Thermo-magnetism he holds to be incapable of producing such strong currents, as the strongest currents are observed exactly where the two points were separated by a non-metallic conductor; and he concludes that there remains only the hydro-electric action of the metallic components of the vein to account for the phenomena. In respect to the extent of the deviation of the multiplier, it is to be borne in mind, that there can be no immediate conclusion drawn from this as to the electric difference of the substances coming into play; for it depends on the resistance to conduction in the entire circuit, which again depends on the dimensions and nature of the intervening rock, as also on the more or less perfect contact between the copper disc and the ore, and between the disc and the wire.

Some Observations on the preparations of Barium and Strontium. By Dr. HARE, *in a Letter to* J. F. W. JOHNSTON, *Esq.*

"Philadelphia, July 4, 1839.

"By means of the alternate action of two deflagrators, each of 100 pairs, containing more than 100 square inches of zinc surface, assisted by refrigeration*, I procured amalgams of barium, strontium, and calcium from their chloride; and by distillation in an iron crucible, included in an air-tight alembic of the same metal, have extricated the metals above mentioned from their mercurial solvent.

"They are so oxidizable, that in order to see their brilliant white metallic colour the eye must follow close upon the track of the file or the burnisher. Almost as soon as a fresh surface is exposed, it assumes a straw colour, like that of iron in the first stage of oxidizement, and is soon completely obscured by the generated oxide. In this way barium and strontium are more ready to oxidize than calcium, although the amalgam of this last-mentioned metal changes much more rapidly in the air. The amalgams of the former metals are more like that of potassium.

"Either metal is rapidly oxidized in water, or in any liquid containing it, and gives afterwards, with tests, the appropriate indication of its presence. They all sink in sulphuric acid. They are all brittle, and fixed, and for fusion require a good red heat.

"Of several kinds of naphtha in my possession, only one, which I have distilled from a residue of the distillation of potassium†, does not act upon the metals above mentioned. After being for some time in naphtha, their effervescence with water is much less active. Under such circumstances they re-act, at first more vivaciously with hydric ether than with water or chlorohydric acid, because the ether removes a resinous coating derived from the naphtha."

On a small Voltaic Battery of extraordinary energy. By W. R. GROVE, *Esq., M.A.*

The author, referring to a communication in the Philosophical Magazine for February 1839, described the preliminary investigations which finally conducted him to the construction of a new battery of unusual power, though of very small dimensions. On the 15th of April, 1839, M. Becquerel presented to the French Academy a small battery constructed by the author, consisting of seven liqueur glasses amalgamated, containing the bowls of common tobacco-pipes, the

* The metals were extricated from saturated solutions of their chlorides. The chemical affinity between the radicals and the oxygen or chlorine in the solution being the opponent of the voltaic action, and this affinity being exalted by heat while the conducting power, and of course susceptibility of decomposition is lessened by the same cause, render resort to a freezing mixture expedient.

† I mean the residue of the receiver, which, agreeably to my process, is an iron tube. See the forthcoming volume of the American Philosophical Society.

metals of zinc and platinum, and the electrolytes concentrated nitric, and diluted muriatic sulphuric acids. This little apparatus produced effects of decomposition equal to the most powerful batteries of the old construction. (*Comptes rendus*, 15 April, and Phil. Mag., May 1839.) In his endeavours to render a construction on this principle practically useful, the author found it economical and advantageous to employ on the platinum side a mixture of concentrated nitric and dilute sulphuric acids as an electrolyte. He also recommends parallelopipedal instead of cylindrical vessels.

"The hastily-constructed battery, which accompanies the paper, consists of an outer case of wood, height $5\frac{1}{2}$ inches, breadth 5, width 3, (it should be of glazed earthenware, similar to the Wollaston troughs,) separated into four compartments by glass divisions; into these compartments are placed four flat porous vessels, the interior dimensions of which are $5 \cdot 2\frac{1}{2}$ and $\frac{3}{10}$ths of an inch, the thickness of the 'parois' $\frac{1}{8}$th of an inch; they contain each three measured ounces. The metals (four pair) expose each a surface of 16 square inches, and the battery gives, by decomposition of acidulated water, 9 cubic inches of mixed gases per minute: charcoal points burn brilliantly, and it heats 6 inches of platinum wire, $\frac{1}{40}$th of an inch diameter; its effect upon the magnet, when arranged as a single pair, is proportionately energetic; it is constant for about an hour without any fresh supply of acids. The porous vessels are identical in their constitution with the common tobacco-pipe.

"As far as my experiments go, its power with reference to Mr. Daniell's battery is, '*cæteris paribus*,' as 16 to 1; but the relative proportions vary somewhat with the series. The cost of the whole apparatus is £2 2*s*.

"During the operation of this battery the nitric acid, by losing successive portions of oxygen, assumes first a yellow, then a green, then a blue colour, and, lastly, becomes perfectly aqueous; hydrogen is now evolved from the platina, the energy lowers, and the action becomes inconstant. It is worthy of remark, as an argument for the secondary nature of metallic precipitation by voltaic electricity, that the oxidated or dissolved zinc remains entirely (or at least by far the greater portion) on the zinc side of the diaphragm, the hydrogen alone appears to be transferred; and yet the reversal of affinities which the theory of reduction by nascent hydrogen supposes, is an enigma difficult of solution.

"I have invariably observed in this battery a current of endosmose from the zinc to the platinum, or with the current of positive electricity.

"The rationale of the action of this combination, according to the chemical theory of galvanism, appears to be as follows: In the common zinc and copper combination, the resulting power is as the affinity of the anion of the electrolyte for zinc minus its affinity for copper; in the constant battery it is as the affinity of the anion for zinc, plus that of oxygen for hydrogen, minus that of oxygen for copper. In the combination in question the resulting power is as the affinity of the anion for zinc, plus that of oxygen for hydrogen,

minus that of oxygen for azote *; nitric acid being much more readily decomposed than sulphate of copper, resistance is lessened, and the power increased; and no hydrogen being evolved from the negative metal, there is no precipitation upon it, and consequently no counter-action.

"I need scarcely add a word as to the importance of improvements of this description in the voltaic battery: this valuable instrument of chemical research is thus made portable, and by increased power in diminished space, its adaptation to mechanical, and especially to locomotive purposes, becomes more feasible."

Notices of Experiments on the deposition of Metals by Voltaic Action. By THOMAS SPENCER, *Esq.* (*Liverpool.*)

Among other experiments made by the author to ascertain the truth of an opinion he had been previously induced to entertain, namely, that if two fluids of different densities were placed as much as possible in mechanical contiguity, that is, without the fluids being suffered to intermingle, an electro-chemical current would be eliminated by the disturbance of the electrical equilibrium, the following was described:

He took a very tall tubular vessel and half-filled it with a solution of nitrate of silver; he then added distilled water in the most careful manner with a pipette, allowing the drops to run down the side of the vessel until he had nearly filled it. By proceeding thus, he was enabled to keep the fluids from intermingling, their density being different. When thus filled, a narrow slip of silver was inserted, long enough to go through both strata of the fluid. The whole was placed in a dark situation, and suffered to remain for a few hours, when evident marks of voltaic action began to manifest themselves. In the course of a few days beautiful metallic crystals had formed on that portion of the silver that was immersed in the solution of the nitrate; that part of the slip situated where the two fluids met, was left to about one eighth of an inch untouched, while that end placed in the acidulated water became oxidized in the inverse ratio of the deposition on the opposite end.

A series of slips of different metals, including zinc, were subjected to similar treatment in their respective solutions, and with like results.

From these experiments Mr. Spencer was led to conclude, that the appearances that are sometimes observed in the mines, and which have been attributed to electrical action, might be simply and satisfactorily explained without supposing the presence of two dissimilar metals, or even metalliferous bodies, as the water found in the mines, and containing salts of the different metals in solution, would, in their action on a *single* metalliferous substance, generate electricity sufficient to account for the deposition of crystals of the pure metal so frequently found on the poorer copper ores.

* I have thrown out of the case the resistance to decomposition of the electrolyte in contact with the zinc, as common to the three combinations.

As an experimental illustration, he took a piece of pretty rich sulphuret of copper and placed it in a narrow glass vessel, half filled with sulphate of copper in solution; the sulphuret was immersed about half its length in this solution; common water, with a few drops of acid, was then added in the manner before stated, taking care that it should not intermingle with the cupreous solution underneath. The whole was then placed in the dark, and left untouched for about a week.

At the end of that time Mr. Spencer examined it, and had the satisfaction of observing that several portions of it had become covered with very minute crytals of the pure metal: at the end of a fortnight still more beautiful crystals had been deposited.

This experiment was repeated with water taken from a copper mine in Anglesey and with similar results.

These experiments were adduced by the author in support of the views of Mr. Fox.

Mr. SPENCER *exhibited a Cylindrical Battery, so as to include great intensity in small space.*

On the Artificial Crystallization of certain Metallic Carburets, as extensive of the Theory of Crystallization. By SAMUEL BROWN, *M.D., Edinburgh.*

The theory of the circumstances in which the phenomenon of crystallization takes place, so far as these have been hitherto known, may be expressed in these three practical maxims:

1st. When a body is slowly reduced to solidity from a state of fluidity (gasiform or liquid), its particles congregate in such a manner as to produce the crystallization of the substance concerned. This includes crystallization by sublimation, fusion, and all the modifications of these processes.

2nd. When a body is slowly deposited in the form of solidity from a state of solution in a fluid (gasiform or liquid), its particles mutually arrange themselves in the crystalline mode of cohesion. This comprehends all cases of simple crystallization, as effected by the gradual elevation or depression of the temperature of solutions, and by the evaporation or dilution of the solvents.

3rd. When an insoluble solid body is slowly formed, whether by synthesis or analysis, *in* a fluid, its particles assume the crystalline disposition about each other. To these the author adds a fourth.

4th. When the particles of a solid body are slowly evolved from the decomposition of a substance of which it, or its elements, are chemical constituents, they cohere in crystal, and that independently, both of the fusion or solution of the body crystallized, and of the presence of any fluid medium of molecular motion whatsoever. *Thus, when an infusible metallic carburet is slowly formed in a shut vessel from the decomposition of the cyanide,* it is procured of a transparent, very hard, and crystalline structure.

The illustration of this general statement, and the exemplification of the instance now cited by anticipation, were the objects of the author's observations.

While engaged in examining the visible properties of these dark, opaque, and uncrystallized products of the decomposition by heat of the cyanides and sulphocyanides of iron, copper, lead, zinc, bismuth, silver, tin, and manganese, specified above, the author noticed on one side of the field of the microscope a minute transparent fragment, close in structure, refracting the light of the mirror peculiarly, and trembling with uncommon brilliancy under the play of the illuminating lens. It was in that prepared from the sulphocyanide of copper. The specimen was searched, and many more such clear morsels found, some of them having even attempted a regular form. These little crystals in all chemical respects conducted themselves in the same manner as the amorphous powder among which they had been found.

Dr. Brown's experiments to determine on what their crystalline structure depended, resulted in establishing the following formula:

Formula. Let a parcel of any cyanide (or sulpho-cyanide) be carefully dried and put in a green glass tube, the open end of which is then to be drawn out, and the attenuated part bent at right angles with the containing. Immerse it horizontally in a shallow sand-bath suspended over a spreading gas flame. Apply the heat with such caution as shall secure that the whole apparatus be always as nearly as possible at the same temperature at the same times. Let the flame be thus progressively raised till the material in the tube has been brought to that degree of temperature which may be called its point of decomposition, which may be indicated in the case of a sulphocyanide by the appearance of bisulphuret of carbon, and in that of a cyanide by the impulse of liberated nitrogen on any light body held over the open extremity of the tube.

Whenever signs of decomposition have been observed, the flame must be lowered as nearly as possible to that degree at which it is able to communicate increments of heat equal to the decrements, by radiation and conduction, of the apparatus. Continue the operation so long as gas is extricated. In this way will the cyanide (or sulphocyanide) have been decomposed, as nearly as manipulation can effect, at its "point of decomposition"; the particles of resultant carburet will have been slowly evolved one after another; and, instead of throwing themselves into a shapeless aggregation, will have affected the regulated arrangement of crystallization.

"1st. Carburets thus crystallized are like sands of large grain; the granules having, many of them, regular forms, generally double octohedrons. Larger crystals may be formed, although I do not know how their formation may be secured.

"2nd. When prepared, they are opaline, or like semi-transparent enamel. On being heated more strongly, they become perfectly clear. I have heated them in the hottest blast of air and blast furnaces for hours without melting them; without, indeed, producing

any effect except that of completing their translucency and improving their appearance.

"3rd. When raised to a red heat in the air, they slowly combine with oxygen, and yield the same products as the amorphous substances. In all respects they comport themselves as the same carburets when uncrystallized, except that, as might have been anticipated from their hardness, they are far less susceptible of reaction. And hence a specimen may be freed of uncrystallized powder by cautious heating in the air, and subsequent washing with the common acids.

"These facts, besides their bearing on geological and other questions, prove that the only essential condition of the crystallization of a body is, that its particles be slowly segregated *from any* previous condition whatever. This will appear to be more important when it is recollected, that in all former experimental crystallizations there seemed to be *two* elements, or essential conditions, viz. slowness of separation of particles, *with* the presence of fluidity in some form or other, as the medium of their easy motion."

Experimental Demonstration of the certain existence of Halöid Salts in Solution. By Dr. George Wilson.

It was observed that all previous attempts to decide the question, whether halöid salts do or do not decompose water, when dissolved in it, had afforded no certain results, since none of the methods employed yielded an *experimentum crucis*. The object of this paper was to show, that though the inquiry had long been abandoned as hopeless, a demonstration can be given of the persistent halöid condition of the dissolved halöid salts of the electro-negative metals. The mode of experimenting followed was based on an important difference between the two great classes of metals. The electro-positive metals, as potassium, sodium, zinc, iron, &c., dissolve in the hydracids with the evolution of hydrogen, indicating thereby (according to the more simple theory of the change) an attraction for the radicals of the acids superior to that of the hydrogen which they displace; the electro-negative metals, on the other hand, do not dissolve in the hydracids, nor decompose them; they cannot, therefore, displace hydrogen, but should be displaced by it. As the halogens or salt radicals have thus a less attraction for the electro-negative metals than hydrogen has, this body may be employed to decompose their halöid salts. The action of uncombined hydrogen on the dissolved salts was shown to be not of itself decisive as to their true state in solution, but its effect on them when evolved from an hydracid affords an unequivocal *experimentum crucis*. Gold and its terchloride were chosen as examples of an electro-negative metal and its halöid salt; and hydrobromic acid was taken as the decomposing re-agent. In anticipating their action on each other, it appeared that if the chloride of gold became by solution in water an hydrochlorate of the oxide, it should not be decomposed by hydrobromic acid, which has a less attraction for all metallic oxides than hydro-

chloric acid has. But if it be persistent as a chloride, it should be decomposed by hydrobromic acid, since the chlorine and hydrogen are combined with two bodies, gold and bromine, for which they have a less attraction than they have for each other; they should therefore combine to form hydrochloric acid, while the relinquished gold and bromine also unite that a bromide of gold may be formed. When this experiment is performed, the chloride *is* decomposed by hydrobromic acid, the bodies added being the dissolved terchloride of gold, and hydrobromic acid; the products of the change are, the similarly dissolved terbromide of gold and hydrochloric acid; and the immediate proof of such a change having occurred, is the alteration of colour which succeeds the mingling of the two bodies. The solution of the chloride of gold has a pale yellow tint; the bromide has a dark red hue; any conversion, therefore, of the chloride into the bromide is indicated by the colour of the liquid passing from pale yellow to dark red. To determine the certainty of this decomposition, the liquid was separated, either by distillation or by agitation with sulphuric ether, into the bromide of gold and hydrochloric acid, which were respectively tested and ascertained to be such.

The certainty of the decomposition having been determined, and its occurence in atomic as well as in irregular proportions learned by repeated experiment, the theory of the change may now be considered.

The salt of gold, being a terchloride, will require three proportions of hydrobromic acid for its complete decomposition. On the supposition of its dissolving as a halöid, its decomposition will occur thus:

Decomposition of Terchloride of Gold by Hydrobromic Acid.

(*The figures express atoms.*)

Terchloride of gold, 1.	Gold, 1	Terbromide of gold, 1.
	Chlorine, 3	
Hydrobromic acid, 3.	Bromine, 3	Hydrochloric acid, 3.
	Hydrogen, 3	

Again, let the liquid be supposed to contain a muriate of gold, it will be thus:

Decomposition of Termuriate of Gold by Hydrobromic Acid.

Termuriate of gold, 1.	Hydrochloric acid, 3	
	Oxygen, 3	Water, 3.
	Gold, 1	
Hydrobromic acid, 3.	Hydrogen, 3	Terbromide of gold, 1.
	Bromine, 3	

According to the former of these views, the hydrogen of the hydrobromic acid takes chlorine from chloride of gold, so that hydrochloric acid is *formed*; according to the latter, hydrogen takes oxygen from the oxide of gold, and water is formed; while hydrochloric acid, previously in combination, is set free.

By a reference to the diagrams it will be seen, that in each of these,

the only two possible methods of explanation, we end by admitting the production and presence of a true halöid salt, the terbromide of gold. It cannot be declared possible that the bromide of gold, as soon as formed, has decomposed water, and become an hydrobromate, since, by reference to the lower diagram, it will be seen that the production of the bromide, according to the Amphide theory, implied the re-solution of hydrobromic acid and oxide of gold into water and bromide of gold. The production of water was declared essential to the change; its decomposition cannot therefore be assumed as equally possible, since the previous half of the explanation would thus be negatived, and the theorist should only exclude the bromide from the list of halöid bodies by admitting the chloride, or *vice versâ*. The nature and value of this mode of experimenting is summed up in the following proposition: The dissolved terchloride of gold is decomposed by hydrobromic acid, with the production of terbromide of gold, in circumstances which make it certain that one of these salts is a halöid in solution: but if one of these be a halöid the other is also, and so are all the salts of the same class.

The demonstration is further shown to be afforded in another way: it will be seen, by reference to both diagrams, that each view recognises the presence of bromide of gold and hydrochloric acid. But if the bromide became a hydrobromate, it would at once be decomposed by the hydrochloric acid; it is permanent, however, and must therefore be a bromide while in solution.

A similar inquiry was stated to have been followed out with the halöid salts of platinum, and with equally decisive results; and the author observed that the method proposed would afford an equally unequivocal demonstration for the halöid salts of all the metals which have a less attraction for hydrogen than the radicals of the hydracids have; *i. e.* it would apply to all the electro-negative metals. It was further observed, as an incidental conclusion from the experiments recorded, that they afforded a direct proof of the quasi-metallic character of hydrogen, so much insisted on by the advocates of the binary theory of salts; and that they supplied more direct evidence than any previous trials regarding this, since they not only demonstrate hydrogen to have the power of displacing many metals, but at the same time assign to it, as its proper place in its metallic character, a position intermediate between the electro-positive and electro-negative metals.

On the limits within which the Atomic Weights of Elementary Bodies have been ascertained. By THOMAS CLARK, *M.D., Professor of Chemistry in Marischal College and University, Aberdeen.* [This paper is given entire at the recommendation of the Committee of the Chemical Section, confirmed by the General Committee.]

The atomic weights of elementary bodies, as they occur in the received tables, commonly represent the mean result, or a selected result, of the experiments on which they are founded. It may be now useful,

at a distance of about thirty years from the period when attempts to ascertain such weights with accuracy were first made, to have placed before us, not merely the mean or selected result of the fundamental experiments, but also their highest and lowest results. The variations are much greater than chemists in general are aware of.

To attempt giving a view of the variations in the atomic weights of each elementary substance would occupy far too much time for an occasion like the present. All that I propose is, by adverting to certain compounds of lead, in illustration of the atomic weight of that metal and of the atomic weights of sulphur, azote, and carbon, to afford some idea of the limits within which the atomic weights of these four elements have been ascertained.

In proceeding to indicate such limits, I need not, in this presence, express how much the selection of safe data requires the exercise of circumspection. The results of only such experiments as have been performed with the utmost care should be employed. By including the results of experiments otherwise conducted, we increase the limits of the recognisable variations, which is the opposite of desirable, without thereby increasing the certainty of the mean or selected result.

The Protoxide of Lead.

The proportions of oxygen and lead in this compound have been repeatedly examined by Berzelius. The mode of analysis ultimately preferred by him, consisted in reducing the oxide by pure hydrogen gas, at an elevated temperature, and weighing the residual metal. The loss was accounted oxygen. In his last and most careful experiments, he obtained, as the result of six experiments, for every 100· of oxygen, the following quantities of lead:

$$\left.\begin{array}{ll}\text{Greatest} & 1295{\cdot}70\\ \text{Mean} & 1294{\cdot}24\\ \text{Least} & 1293{\cdot}08\end{array}\right\}\text{Weighed in air.}$$

But inasmuch as, weight for weight, the protoxide of lead is more bulky than lead itself, the oxide would, when weighed in air, be proportionably more buoyed-up than the metal. Thus if 1394· grains of lead were balanced in air against 1394· grains of its protoxide, the same 1394· grains of lead would, when transferred to a vacuum, counterpoise 1394·033 grains of the same protoxide. Hence, in the foregoing results, the oxygen, instead of 100·, should be reckoned 100·033. By reducing this 100·033 to 100·, and by reducing the lead in the same proportion, we obtain of lead to every 100· of oxygen,

$$\left.\begin{array}{ll}\text{Greatest} & 1295{\cdot}27\\ \text{Mean} & 1293{\cdot}82\\ \text{Least} & 1292{\cdot}65\end{array}\right\}\text{Weighed in a vacuum.}$$

Berzelius has, more than once, observed that the circumstance of the atomic weight of lead being about thirteen times the atomic weight of oxygen, causes the slightest error in experiment to be very much multiplied in calculation. Thus the difference between the highest and

the lowest atomic weight of lead is 2·62; yet this difference, great as it seems, results from a difference of only one 74th of a grain in the lead left from 100· grains of the protoxide. Considering that this difference represents the widest variation that occurred in the experiments, I need not say how much accuracy the smallness of its extent implies. It is calculation that so much magnifies this inconsiderable experimental difference, and in two ways—first, whatever is subtracted from the lead comes to be added to the oxygen, and the converse; and second, the difference in the oxygen comes, when computed on the lead, to be multiplied by about thirteen times, the atomic weight of lead being thus much greater than the atomic weight of oxygen; so that, in these two ways, an error of only *one* in 7400· on the protoxide increases to 2·62 on 1394· of the protoxide, or on 1294· of lead, or to about *one* in every 500· of the metal. Thus the atomic weight of lead comes to vary in a ratio fifteen times as much as the experiment whence it is derived. Certainly it is an unhappy circumstance when calculation so much magnifies the unavoidable errors of experiment, more especially when, as will be seen presently to happen in the instance of lead, the original variation comes to be complexified with other variations, and the error in atomic weight comes to be communicated in aggravated amount from element to element.

The Sulphate and the Nitrate of Lead.

Berzelius converted known weights of pure lead into a solution of the nitrate, and, adding thereto an excess of dilute sulphuric acid, he evaporated the whole to dryness, and heated the mass to redness, so as to obtain the dry sulphate of lead. Turner repeated the experiment nearly in the same manner; but he made allowances for some slight corrections that were neglected by Berzelius. The weight acquired by the lead, in becoming the sulphate, should be increased on account of the buoyancy of the air, and should be diminished on account of the solid impurities of the acids employed.

On 100· of lead the correction for buoyancy is	+·0125
for acid impurities	−·0190 (Turner.)
Nett correction	−·0065

Making allowance for this correction in the experiments of Berzelius, 100· of lead afforded of the sulphate,

Berzelius.		Turner.	
146·374	Mean 146·401	146·430	Mean 146·401
146·396		146·398	
146·433		146·375	
146·451			

The experiments of Turner, taken along with the three first of Berzelius's, would seem to indicate that the last experiment of Berzelius's affords too high an increase. From a comparison of the results obtained by both chemists, the following quantities have been adopted in

the calculations in the sequel, as the experimental increase on 100· of lead in becoming the sulphate:

Greatest	46·431
Mean	46·401
Least	46·374

A known weight of dried nitrate of lead was likewise, by Turner, dissolved and heated with sulphuric acid, so as to form sulphate. Corrections on account of the buoyancy of the air and of acid-impurities having been made, 100· of sulphate of lead were produced from—

	Nitrate.
1,	109·312
2,	109·310
3,	109·300
Mean	109·307

These results come very near each other. But according as 100· of lead, in becoming the sulphate, increases 46·431, 46·401, or 46·374, the proportion of lead in its nitrate will vary, on account of the uncertainty of the composition of the resulting sulphate, altogether independent of the small variation in the proportion of nitrate that produces a given weight of sulphate.

			Greatest.	Mean.	Least.
Sulphate from	100·	Sulphate from	146·431	146·401	146·374
Nitrate	109·312	Nitrate	160·067	160·035	160·005
	109·310		160·065	160·032	160·002
	109·300		160·050	160·017	159·987
(Mean)	109·3075		160·060	160·027	159·997

146·431, 146·401, 146·374, being the quantities of the sulphate of lead that were formed from 100· of lead, it follows that the quantities of nitrate 160·067.........159·987 should likewise contain 100· of lead.

Bringing then together the results of the experiments on the sulphate and on the nitrate, it appears that the following are the increments of weight that 100· of lead acquires in becoming,

	Sulphate.		Nitrate.
Greatest	46·431	. . . .	60·067
Mean	46·401	. . . .	60·027
Least	46·374	. . . .	59·997

The increase of weight that lead acquires in becoming sulphate, is generally admitted to be due to the acquisition of one atom of sulphur and four atoms of oxygen. In the case of the nitrate, however, while the generality of chemists regard the oxygen acquired to be six atoms, the azote is, by some, considered as one atom; by others, as two. With Dalton and Berzelius, I regard the azote as constituting two atoms.

The increase, then, acquired by one atom of lead, will, in the case of the sulphate, be the sum of one atom of sulphur and four atoms of oxygen (S^1O_4), and, in the case of the nitrate, be the sum of two atoms of azote and six atoms of oxygen (A^2O_6). Starting from the atomic weight of lead, varying as already given, the following table exhibits the varying experimental increments corresponding to S^1O_4 and to A^2O_6:

		Greatest.	Mean.	Least.
On LEAD one atom,		1295·27	1293·82	1292·65
	Increase.			
S^1O_4	Greatest	601·41	601·02	600·67
	Mean	600·73	600·34	599·99
	Least	600·19	599·80	599·45
A^2O_6	Greatest	778·03	777·15	776·46
	Mean	777·51	776·64	775·94
	Least	776·99	776·12	775·42

Deducting 400· for the oxygen in the sulphate, and 600· for the oxygen in the nitrate, we obtain the following atomic weights:

	Greatest.	Mean.	Least.
Lead, 1 atom. ...	1295·27	1293·82	1292·65
Sulphur, 1 atom. ...	201·41	200·34	199·45
Azote, 2 atoms. ...	178·03	176·64	175·42

The differences between the experimental atomic weights of each of these elements are

	Between mean and extremes.	Between extremes.
Lead	+1·45 − 1·17	2·62
Sulphur	+1·07 − ·89	1·96
Azote (A^2)	+1·39 − 1·22	2·61

These differences are considerable. They show that there is an uncertainty of $\pm$ 1· in the place of units, in each equivalent, oxygen being assumed at 100·. From the manner in which the atomic weights have been sought, the difference in the instance of sulphur is evidently due partly to the variations found in the atomic weight of the lead, and partly to the variation in the increase that lead acquires in becoming sulphate; and the difference in the instance of azote is due not only to the same two variations, but also to the varying product of the sulphate from a given weight of the nitrate. What portion of the differences between the mean and each extreme is due to each of these causes, in the equivalents of sulphur and of azote, appears in the following table:

	In sulphur.	In azote.
On account of the oxide	+·68 −·54	+·87 −·70
„ „ sulphate	+·39 −·35	+·42 −·39
„ „ nitrate		+·10 −·13
	+1·07 −·89	+1·39 −1·22

The differences on account of the oxide are the most considerable; next those on account of the sulphate; and the differences on both of these accounts are ten times greater than what arises from the experiments on the nitrate alone. Thus, in such researches, do errors cumulate.

On reviewing the atomic weights thus ascertained, together with their variations, there appear to me to be considerations entitling us to reject some of the highest.

First, as to azote. A^2 has been found as high as 178·03. But admitting the atmosphere to consist of 21 bulks of oxygen for 79 bulks of azote, we are quite sure that A^2 cannot be so much as 177·; for then the specific gravity of oxygen must be less than 1·100, which would contradict all good experiments on the subject that have been made. Therefore 177· and any number higher must be rejected as too much for A^2.

Next as to lead. By calculating from 1295·27, the greatest atomic weight of lead, we obtain for A^2, 178·03, 177·51, 176·99. But as even this last and smallest number has been shown to be too high for azote, so must 1295·27 be too high for lead. The next lower number found by Berzelius was 1294·52, which affords,

	A^2O_6	A^2
Greatest	777·58	177·58
Mean	777·06	177·06
Least	776·54	176·54

But this mean and this greatest are evidently too high, for the reason already mentioned. Even the smallest number (176·54 for A^2) corresponds with a specific gravity for oxygen of 1·1021, which is below the received experimental results. Therefore I apprehend we are entitled to reject the second as well as the highest of Berzelius's experimental results for the atomic weight of lead. His remaining experiments are four:

1.	1293·775
2.	1292·647
4.	1292·795
5.	1293·888
Mean	1293·276

Say,

Highest	1293·89
Mean	1293·27
Least	1292·65

Repeating the calculations on this basis, we have,

	Greatest.	Mean.	Least.
On lead one atom	1293·89	1293·27	1292·65
Increase.			
S^1O_4 Greatest	600·77	600·48	600·19
S^1O_4 Mean	600·38	600·09	599·80
S^1O_4 Least	600·03	599·74	599·45

		Greatest.	Mean.	Least.
A^2O_6	Greatest	777·20	776·83	776·46
	Mean	776·68	776·31	775·94
	Least	776·16	775·79	775·42

The atomic weights hence deduced are,

	Greatest.	Mean.	Least.
Lead	1293·89	1293·27	1292·65
Sulphur	200·77	200·09	199·45
Azote (A^2)	177·20	176·31	175·42

The variations now, and the sources of them, are shown as follows:

	In sulphur.		In azote.	
On account of the oxide	+·29	—·29	+·42	—·39
” ” sulphate	+·39	—·35	+·37	—·37
” ” nitrate			+·10	—·13
	+·68	—·64	+·89	—·89

Tartrate and Racemate of Lead.

These two salts, containing the same ultimate elements, were carefully examined by Berzelius. His results are contained in the following table, which is constructed on the same principle as those for the sulphate and the nitrate:

		Greatest.	Mean.	Least.
	Protoxide	1393·89	1393·27	1392·65
Acid. $4\,C+2\,H^2+5\,O$	Greatest	828·16	828·53	828·90
	Mean	827·23	827·60	827·97
	Least	826·30	828·67	827·04

Allowing 12·6 for H^2, which I consider to be the result warranted by Berzelius's experiments, the following would be

The atomic weight of carbon:

Greatest	75·92
Mean	75·60
Least	75·28

On the relative Combinations of the Constituents of Cast Iron, Steel, and Malleable Iron. By Dr. CHARLES SCHAFHAEUTL, *of Munich.*

The author showed, that the purest carbon contained and retained hydrogen, and sometimes azote, even at the highest temperatures, and parted with neither of them, nor were its own internal and external properties altered, except when it attacked the crucible, and combined instead with oxygen, or aluminum, or silicon. He affirmed, that we possessed no certain method of procuring pure carbon in the isolated state, and that what we considered to be pure carbon was always, more or less, in the state of *carburet.* The author described a new method of obtaining graphite, viz. by running fluid puddling slag, or silicates

of iron and manganese, over fragments of pit coal. After cooling, the surface of the slag is always found to be altered, and to be covered with a very easily separable layer of graphite, not only where the slag actually touches the coal, but even where it comes in contact with the smoke evolved from the coal. The formation of graphite commences at a temperature lower than 1500° Fahr., and reaches its highest point not much exceeding 2000°. Two different sorts of graphite were produced in this way; one, which he marked (A), was in elastic scales, of the thickness of writing-paper, with a rather dull metallic appearance. The graphite marked (B) was of the thickness of gold-leaf, and extremely light and unctuous to the touch. He found, that all sorts of graphite lost their unctuosity and bright appearance by exposing them to the action of concentrated hydro-fluoric acid. Graphite (B) was found to consist of

Protoxide of iron	18·6000
Silica	7·6200

probably mechanically, but equally and invisibly intermixed with

Carbon	70·3421
Silicon................	3·0744
Loss	00·3635
	100·0000

Graphite (A) gave,

4·93	Silicon.
9·50	Iron.
85·45	Carbon.
00·12	Loss.
100·00	

The quantity of oxides of iron and silica had been ascertained by heating the specimens first with acids and caustic leys; the quantity of carbon, by burning the specimens with chromate of lead and chlorate of potash; and the silicon, by melting the powders with carbonate of soda in a platinum crucible. He considered, therefore, the graphite to be a *carburet of silicon* and iron; and showed, by heating in a *peculiar way* the remainders, left after the solution of iron in hydrochloric acid of a certain specific gravity, that the chemical composition of cast iron, in its two distinct species of gray and white cast iron, had direct relation to the two specimens of graphite, and in all probability was derived from similar origin, as indicated in the following table:

Graphite (B).		Gray Cast Iron.	
Iron, Silicon	Oxygen. Silicate of iron.	Iron, Silicon, (Aluminum)	Silicet and aluminet of iron.
Silicon, Carbon	Carburet of silicon.	Carbon, Silicon	Carburet of iron.

Graphite (A).		White Cast Iron.	
Iron, Carbon	Carburet of iron.	Iron, Carbon	Carburet of iron.
Carbon, Silicon	Carburet of silicon.	Azote, Silicon, Carbon	Carburet of silicon.

It was further shown, that all gray iron, produced by heated air as well as by cold air, left a grayish white residue behind after treating it with hydrochloric acid of a certain specific gravity. This remainder, acted upon with caustic ammonia, evolved very rapidly pure hydrogen gas, and alumina afterwards was found in the solution with a little silica. The presence of aluminum in its metallic state, after having been treated with acid, as well as the absence of all *azote*, seemed to be one principal feature *of gray iron of France* as well as of England; on the contrary, carbon, hydrogen, and *azote* are always present in the remainders of *white iron*, which remainders appear invariably of a brownish colour; and azote is a constituent of steel as well as of wrought iron. Further, it was explained, that silicon generally was combined with *carbon*, and dissolved in the carburets of iron, and that it was extremely difficult to produce an alloy of iron with silicon alone, without the presence of a little carbon, aluminum, and other similar bodies. Dr. Schafhaeutl found the molecules of all iron of a similar form, belonging to the cubical system, and the largest not exceeding 0·0000633 of an inch in diameter, and that particularly upon the arrangement of these molecules depends, in a great measure, the different appearance of the different kinds. He denied that any graphite scales were to be seen in gray cast iron ; yet, that under a magnifying glass what appeared to the naked eye graphite scales, were really surfaces and planes of crystallization, composed of pentagonal planes not wider in the smallest diameter than 0·000355 of an inch, and composed of the before-mentioned smallest or primitive iron molecules. According to his statement, the molecules of the iron are arranged in the gray cast iron in the most regular form, having all their surfaces in continuous planes ; the most equal distribution of molecules appeared in hardened steel ; collecting in fascicular aggregation in soft steel, and being loose and longitudinally arranged in wrought iron. He stated that pure iron could not be welded ; that the welding power of iron depended on its alloy with the carburet of silicon, and also that the good and various qualities of all the wrought irons depended on the alloys of pure iron with other metallic bodies ; and that the presence of most of the electro-negative metals had been generally overlooked in the published analyses of iron. The presence of arsenic in Swedish steel, when forged red hot, could be ascertained by its smell, as well as in the Low Moor iron. The usual solution of iron under analysis, in order to separate those metals from the iron, must be, for the necessary correction, divided into two parts,—one to be treated with a current of sulphuretted hydrogen, the other part *dropped* into the sulphydrate of ammonia, and carefully digested. A small quantity of silica was more difficult to separate from a large quantity of iron than generally seemed to be

believed; and the real amount of carbon could *only* be ascertained by Berzelius's method of burning iron in a current of oxygen, or mixed with chlorate of potash and chromate of lead in a glass tube, used first by Berzelius for analysis of organic bodies.

The author maintained that steel was an entirely mechanical production of the forge hammer, which tore the molecules of certain species of white cast iron out of their original position, into which the forces of attraction, in respect to the centres as well as to the position of the molecules, had arranged those molecules by the slow action of heat. Steel, as it came out of the converting furnace or the crucible, was nothing more or less than white cast iron, of which Indian steel, called Wootz, was the fairest specimen.

The author finally gave an analysis of two specimens of cast iron and one of steel. The first specimen was French gray iron, from Vienne, Department de l'Isere, obtained from a mixture of pea-iron-ore with red hematite, by means of coal from Rive de Gier and heated air, specific gravity 6·898. The second iron was Welsh iron, from the tinplate manufactory of the Maesteg ironworks, near Neath, in South Wales, obtained from a mixture of clay iron-stone and Cumberland red ore, by means of coke and heated air. It was silvery white, without signs of crystallization: specific gravity 7·467. The third specimen was a fragment of a razor forged in the author's presence, in the workshop of Mr. Rodgers, of Sheffield, of the specific gravity of 7·92.

	Gray French iron.	White Welsh iron.	Steel.
Silicon	4·86430	1·00867	0·52043
Aluminum	1·00738	0·08571	0·00000
Manganese	0·75130	traces.	1·92000
Arsenic	0·00000	0·00000	0·93400
Antimony	0·00000	1·59710	0·12100
Tin	0·00000	0·00000	traces.
Phosphorus	0·54000	0·08553	0·00000
Sulphur	0·17740	0·32018	1·00200
Azote	0·00000	0·76371	0·18310
Carbon	3·38000	4·30000	1·42800
Iron	89·00740	91·52282	93·79765
Loss	00·27222	00·31428	0·09382
	100·00000	100·00000	100·00000

On the Composition of Idocrase. By Mr. T. Richardson.

With the view of assisting in explaining the discrepancies regarding the composition of idocrase, which exist in our best chemical works, the author presented in this communication the result of five analyses of this mineral from specimens furnished to him by Mr. Wm. Hutton.

1. *Specimen from Egg in Norway.*

The colour was olive-green; lustre vitreous, semi-transparent, and fracture uneven. It was analysed in the usual way, every precaution

being taken to insure accurate results, and the composition obtained was as follows:

Silica	38·75	contained	....		20·130 oxygen.
Alumina	17·35	———		8·102	19·979,
Protox. iron	8·10	———	1·843	11·859	
Lime	33·60	———	9·436		
Magnesia	1·50	———	·580		
	99·30				

which is most accurately represented by the formula,

$$7\ (FO\ CaO\ MgO)_3\ Sil_3 + 5\ Al_2O_3,\ SiO_3.$$

2. *Specimen from Slatoush in Siberia.*

Colour light yellow-green; vitreous lustre; streak white; granular composition, and the individuals slightly connected, semi-transparent. The result of the analysis approaches very closely that of magnus and turrenkass, and differs completely from that of Mr. Ivanoe. It contained as follows:

Silica	37·45	contained	....		19·454 oxygen.
Alumina	18·85	———		8·662	20·849,
Protox. iron	7·75	———	1·764		
Protox. manganese		———		12·187	
Lime	35·25	———	9·901		
Magnesia	1·35	———	·522		
	100·35				

which may be represented by the same formula as the above.

3. *Specimen from Piedmont.*

Colour sage-green; vitreous lustre; semi-transparent; in part massive, terminating in small crystals; fracture uneven. The result of the analysis was,

Silica	39·25	containing	....		20·390 oxygen.
Alumina	17·30	———		8·079	19·902.
Protox. iron	7·62	———	1·695		
Protox. man.	3·50	———	·784	11·823	
Lime	32·25	———	9·057		
Magnesia	·47	———	·287		
	100·39				

This also corresponds with the former.

4. *Specimen of Vesuvian from Monte Somma.*

It possessed the following characters: colour olive-green; lustre

vitreous, semi-transparent, well crystallized. The composition was,

Silica	37·90	containing				19·688 oxygen.
Alumina	18·15	———			8·475	20·578.
Protox. iron	4·89	———	1·112	12·103		
Lime	34·69	———	9·742			
Magnesia & Manganese	3·23	———	1·249			
	98·86					

5. *Specimen of Egerane from Eger in Bohemia.*

Its characters agreed with those of the other specimens, with the exception of its colour which was cinnamon-brown, and its opacity. The result of the analysis was,

Silica	38·40	containing				18·819 oxygen.
Alumina	18·15	———			8·475	20·620.
Protox. iron	7·40	———	1·684	12·145		
Prot. manganese						
Lime	33·09	———	9·293			
Magnesia	3·02	———	1·168			
	100·06					

The inference from the whole of these analyses is, that the composition of idocrase may be represented by the formula before given, viz.

$$7\ (FO\ CaO\ MgO\ MO)_3\ SiO_3 + 5\ Al_2\ O_3\ SiO_3,$$

which may also be referred back to the fundamental formula of the garnet, $3\ \dot{R}O\ SiO_3 + R_2O_3, SiO_3$. This conclusion suggests the idea, that by attending more minutely to the exact representation of the analytical results in the formula, new light might possibly be thrown upon some points in the doctrine of Isomorphism.

Some observations on Meteoric Iron found in different parts of the United States of America. By CHARLES UPHAM SHEPARD, *M.D., Professor of Chemistry in the Medical College of the State of South Carolina, and Lecturer on Natural History in Yale College, Connecticut.*

During the last winter Dr. Hardy of Ashville, North Carolina, presented to the author a mass of apparently native iron, weighing seven or eight ounces, that had been detached from a ball of about five inches in diameter, which was found loose in the soil about five miles west of Ashville, in Buncombe county, North Carolina. The specimen evinced a decidedly crystalline structure, and even approached, in general figure, that of a flattened octohedron. Its surface had a dissected or pitted appearance, occasioned by the removal of portions of the external laminæ during its separation from the original mass. The cells and cavities thus apparent were perfectly geometrical in shape, being either

rhomboidal, tetrahedral, or in the figure of four-sided pyramids. It required the application of numerous and powerful blows to disengage fragments from the specimen. The hammer slightly indented the surface, and at length loosened sections of the external laminæ, which were detached by the aid of forceps. Their shape was commonly that of acute rhomboids, considerably flattened in their dimensions, but capable of an easy division into regular octohedrons and tetrahedrons, whose exactness of form rivalled the cleavage crystals of fluor. Some of the plates separated into leaves nearly as thin as mica, which substance they even resembled in colour, (being silver white, inclining to steel grey,) and were slightly elastic, though when twisted up they retained their spiral form.

Prior to the separation of any fragments, the surface of the mass did not afford the metallic lustre, but was coated by a thin blebby pellicle, apparently of hydrous peroxide of iron. Those surfaces which were exposed by cleavage lost their silvery grey lustre in the course of a few weeks, and finally presented a rusty exterior, and even exfoliated spontaneously, and separated into thinner layers and fragments.

In specific gravity the fragments at first detached yielded various results, from 6·5 to 7·5, and even 8·0,—a diversity no doubt dependent mainly on the compression of the fragments produced during their separation from the mass.

It was not until several days after Dr. Shepard commenced the chemical examination of the specimen, that it occurred to him that chlorine might be an ingredient in its composition. Its existence, however, became immediately apparent on the application of the usual test. Nor was he less surprised on discovering, that after repeated digestions of several hours at a time in aqua regia, a dark brown powder remained behind, which was no longer diminished in quantity by a continuation of the process. It was separated from the solution, and ignited with hydrate of potassa in a silver crucible; water was then effused, and the solution subsequently treated with nitric acid. A transparent solution was instantly formed, from which ammonia threw down flocculi of silicic acid, coloured by peroxide of iron. A solution of potassa was now added, and the peroxide of iron separated by the filter. The clear liquid was rendered acid a second time, after which the addition of ammonia threw down white flocculi of silicic acid.

It was in this way that Dr. Shepard satisfied himself of the existence of *silicon* in the Ashville meteoric iron, which element, so far as he was informed, had never before been noticed in an unoxygenated state in any natural body, either meteoric or terrestrial.

The following is a summary of the investigation in regard to this meteoric iron:

Iron	96·5
Nickel	2·6
Silicon	0·5
Chlorine	0·2
	99·8,

with traces of chromium, sulphur, cobalt, and possibly of arsenic and phosphorus.

The cabinet of Yale College contains one of the largest masses of meteoric iron perhaps to be found in any collection. It was obtained above twenty years ago from Texas, near the ridge between the Red River and the Rio Bravo, at a place near the Pawnee village, situated fifteen hundred miles above the confluence of the Red River with the Mississippi. The greatest linear dimensions of the mass in question are the following: three and a half feet long by three feet broad, and two a half in height. It weighs between sixteen and seventeen hundred pounds. Its surface exhibits but slight tendency to oxidation, and presents numerous points of bright lustre.

The author ascertained, many years ago, from small fragments detached from its exterior, that it contained about 9·6 per cent of nickel; and Mr. Benjamin Silliman, jun., having lately had occasion to saw off a large plate from one extremity in order to engrave the donor's name upon the mass, was struck with the highly crystalline appearance of its more internal portions, and with the rapid tendency to oxidation which such parts exhibited. This led him to detach small fragments and test for chlorine, of whose presence he easily obtained the usual evidence.

On the Synthetical Composition of White Prussiate of Potash. By R. Phillips, *F.R.S.*

On the existence of Fluoric Acid as a constituent of certain Animal Substances. By G. O. Rees, *M.D., F.G.S., &c.*

After the statements relating to this subject, published by Morichini, Fourcroy, Vauquelin and Berzelius, Dr. Rees noticed the motives by which he was led to search particularly for fluoride of calcium. His experiments were conducted in the usual manner, by trying to obtain the corroding action of fluoric acid on a plate of glass which was used as a loose cover to a platinum crucible, containing the substance for examination, mixed with strong sulphuric acid, a gentle heat being applied to the bottom of the crucible. In this way several specimens of human bone (both calcined and uncalcined) were subjected to experiment, but in no instance could he obtain any action upon the glass.

The experiment which Berzelius recommends, in order to obtain the corrosion of glass from bone earth, is to distil equal parts of strong sulphuric acid and water upon it until the measure of water is brought over. He states that the distilled liquor, if evaporated in the glass receiver, will produce a corrosion. Dr. Rees repeated this experiment, using 100 grs. of bone ash, and an ounce of the acid mixture, but could obtain no action on the receiver by evaporating the distilled liquor, nor was there any corrosion or opacity produced on any part of the apparatus.

During the evaporation of the last portions of the *liquor*, dense white fumes appeared, and there was some difficulty in vaporising the whole.

On neutralizing a portion with ammonia, and testing with nitrate of silver, a yellow precipitate of phosphate of silver was thrown down. A further examination showed the presence of sulphuric acid, and traces of hydrochloric acid.

Finding phosphoric acid in this result of aqueous distillation, at a moderate heat, the author suggested the possibility of its causing a fallacy in the above mode of testing for fluorine, as it is well known that glass of inferior quality will be corroded by the vapour of phosphoric acid, and all the animal substances in which fluorine has been said to exist, are particularly rich in phosphoric acid.

Having failed to detect fluorine in bone, Dr. Rees determined on testing for it in the enamel of teeth, in recent ivory, and in the precipitate obtained from the urine by means of lime-water. Two different specimens of ivory gave no evidence of the presence of fluoric acid, when carefully tested both before and after calcination; and the author was equally unsuccessful with the enamel of teeth and the precipitate obtained from the urine by lime-water.

"In these experiments, when there was no action upon the glass, it was always found that the addition of 0·3 gr. of fluoride of calcium produced a strong and indelible stain."

Dr. Rees observes in conclusion, "I must express my firm conviction, that if fluoride of calcium be an ingredient of fossil ivory, it must be regarded as an extraneous matter introduced during mineralization; and that no such constituent exists in *recent* ivory, the enamel of teeth, human bones or urine.

"Since writing the above, I have had an opportunity of experimenting on a specimen of FOSSIL ivory, and have succeeded in obtaining evidence of the presence of fluorine. I could not however ascertain the locality from which this specimen was procured. When digested with strong sulphuric acid, at a gentle heat, it produced a rapid and indelible stain on the plate of glass used for the experiment.

Description of an Apparatus for the Analysis of Organic Substances. By Prof. HESS, *of St. Petersburgh.*

The author proposed a modification of Prof. Liebig's apparatus, for the analysis of organic substances, and stated it to be particularly useful in the decomposition of substances which are susceptible of but slow combustion,—of resins, fatty substances, liquids not very volatile, and particularly of solid bodies. The only difference in the mode of conducting the analysis from that adopted by Liebig is the attachment at both ends of the tube for combustion of a caustic potash apparatus, and at one end of a gas-holder containing oxygen, upon the principle originally introduced by Dr. Prout. A description of the apparatus has been published in the *Bulletin Scientifique*, of St. Petersburgh, accompanied by a figure. The heat is communicated by means of a lamp, described and figured by Prof. Hess, in Liebig's *Annalen der Pharmacie* for 1838.

On the Proofs of the existence of free Muriatic Acid in the Stomach during Digestion. By Dr. R. D. Thomson.

The object of this communication was, to offer some experiments which would appear to call in question the principle upon which those were founded, from which Dr. Prout, and Messrs Tiedeman and Gmelin came to the conclusion, that free muriatic acid exists in the stomach of animals during digestion. To show that this point should not be conceded, Dr. Thomson stated the following facts: Dr. Thomson was first led to doubt the statement that free muriatic acid exists in the stomach, from the circumstance of finding muriatic acid not so efficacious as sulphuric in the treatment of alkaline indigestion. He was then induced to examine the nature of the experiments of Dr. Prout, and has arrived at results which seem to render them questionable, if not to disprove the grounds upon which Dr. Prout arrived at the conclusion, that free muriatic acid exists in the stomach of animals. Having obtained a quantity of fluid from the stomach, he evaporated it, and, according to the process of Prout, ignited the residue. On dissolving the residue in water, and evaporating spontaneously, fine crystals of carbonate of soda were obtained along with numerous crystals of common salt. These could proceed only from one of two sources, viz. the decomposition of lactate of soda, should any have existed in the fluid, or from the decomposition of common salt. The author was inclined to attribute it to the latter source rather than to the former, in consequence of the very considerable number of crystals. He had further found, in pursuing his experiments, and in accordance with this result, that certain organic substances possessed the property of decomposing common salt; when tartaric acid and common salt are heated together, copious fumes of muriatic acid are given out. The same phenomenon occurs with citric and oxalic acids: sago also would appear to produce some decomposition, and also saliva; but the latter experiment requires so much delicacy, that Dr. Thomson could not affirm the fact with certainty.

On the Elementary Constitution of Organic Substances. By the Rev. T. Exley, *A.M.*

Referring to the deductions from his hypothesis of the atomic constitution of matter, which regard the union of atoms, and groups of atoms, the author represented graphically, as well as by symbols, the atomic arrangements in several substances which undergo remarkable chemical changes. For example, in organic compounds, of many of which water and olefiant gas are the true sources; as,

1. Pyroxylic spirit .... $H\ (H_2\ O)\ C\ H$
2. Alcohol .... $H_2\ C\ (H_2\ O)\ C\ H_2$
3. Ether .... $H_4\ C_2\ (H_2\ O)\ C_2\ H_4$
4. Valerianic acid .... $H_8\ C_4\ (C_2\ O_3\ H_2)\ C_4\ H_8$
5. Ethal .... $H_{16}\ C_8\ (H_2\ O)\ C_8\ H_{16}$
6. Sulpho-acetic acid ...... $H_{32}\ C_{16}\ (SO_3\ H_2\ O)\ C_{16}\ H_{32}$
7. Stearine .... $H_{64}\ C_{32}\ (C_3\ H_6\ CO)\ C_{32}\ H_{64}$.

The author remarks that there are at least ten substances found whose extremes are the same as No. 1, thirty-three the same as No. 2, and nineteen the same as No. 3. Of the others only a few have as yet been discovered. The calculation of the specific gravity of those in the gaseous form agrees with experiment in a manner which induces the author to urge upon chemists the examination of the hypothesis from which the graphical and symbolical results which he produced were derived.

The extremes after the first are successively doubled, each making exactly one volume, to which the middle atoms, within the parenthesis, contribute nothing: this remarkable result, the author observes, is confirmed in all cases determined by experiment.

Experiments on Fermentation, with some general remarks. By Dr. Ure.

A dispute having taken place between some distillers in Ireland, and officers of Excise, concerning the formation of alcohol in the vats or tuns by spontaneous fermentation, without the presence of yeast, the Commissioners of Excise thought fit to cause a series of experiments to be made upon the subject, and they were placed under Dr. Ure's general superintendence. An experiment made on the 6th of October, 1837, and another experiment commenced on the 12th of October, prove beyond all doubt, that much alcohol may be generated in grain worts, without the addition of yeast, and that also at an early period; but the fermentation is never so active as with yeast, nor does it continue so long, or proceed to nearly the same degree of attenuation.

By employing a peculiar mash-tub, which he had devised, Dr. Ure succeeded in raising the produce of spirit by this process to a perfect accordance with the Excise tables.

The next experiments were made with a view of determining at what elevation of temperature the activity or efficiency of yeast would be paralysed, and how far the attenuation of worts could be pushed within six hours, which is the time limited by law for worts to be collected into the tun, from the time of beginning to run from the coolers.

It would appear from two experiments, that yeast to the amount of 5 per cent. is so powerfully affected by strong worts heated to 120°, as to have its fermentative energy destroyed; but that when yeast is added to the amount of 10 per cent., the 5 parts of excess are not permanently decomposed, but have their activity merely suspended till the saccharine liquid falls to a temperature compatible with fermentation.

Yeast, according to Dr. Ure's observations, when viewed in a good achromatic microscope, consists altogether of translucent spherical and spheroidal particles, each of about the 6000th part of an inch in diameter. When the beer in which they float is washed away with a little water, they are seen to be colourless; their yellowish tint, when they are examined directly from the fermenting square or round of a porter-brewery, being due to the infusion of the brown malt. The yeast of a square newly set seems to consist of particles smaller than those of

older yeast, but the difference of size is not considerable. The researches of Schulz, Cagniard de la Tour, and Schwann, appear to show that the vinous fermentation, and the putrefaction of animal matters—processes which have been hitherto considered as belonging entirely to the domain of chemical affinity—are essentially the results of an organic development of living beings.

Dr. Ure described at length the experimental processes by which this position appears to be established.

On the theory of the formation of White Lead. By Mr. Benson.

The author, after describing at length the ancient and modern processes for preparing white lead, proposed some new views of the operation. The carbonate of lead formed by the common process is anhydrous, amorphous, and contains one proportional each of carbonic acid, oxygen, and lead. Now, as litharge is a protoxide of lead, it has been thought, that in order to effect its conversion into white lead, nothing more was requisite than to combine it with a due proportion of carbonic acid; and from this mistake a variety of fallacious processes have been projected. The processes alluded to are founded upon bringing the litharge into solution as a basic salt, and then precipitating it as a carbonate by the injection of carbonic acid. Painters maintained, that this precipitate was not white lead. Chemists, finding, by analysis, the correct proportions of protoxide of lead and carbonic acid, attributed the opinion of painters to prejudice; but, by microscopic observations, Dr. Ure has ascertained, that the carbonate obtained by precipitation is semi-crystalline, and to a certain degree transparent. The difference between white lead and precipitated carbonate may be illustrated by comparing them to pulverized chalk and powdered marble; both are carbonates, but the one is crystalline, the other is not, and one is, consequently, less opaque than the other; and this difference is, of course, more appreciable, when the powders are diffused through highly refracting media, such as oil. There is one mode by which this difficulty may be avoided. The rationale of both the processes, of that which produces the crystalline, and of that which produces the amorphous carbonate, is the same. In both, the lead is converted into basic acetate—in both, the salt is decomposed by carbonic acid, but in the former the process is modified by the pressure of water. In the one, the carbonate has been deposited from a solution—in the other, the particles, never having departed from the solid state, have not been at liberty to arrange themselves symmetrically. In order, therefore, to produce amorphous carbonate, or white lead, from litharge, it became necessary to present to the oxide of lead a quantity of acetic acid so minute, that an insoluble basic salt should be formed, with a quantity of moisture merely sufficient to determine the action of the carbonic acid. The process would then resemble, in all respects, the ordinary one, except that in the one the lead has been previously converted into oxide, in the other, the formation of the

oxide goes on simultaneously with that of the carbonate. The process has been carried out on a scale of considerable magnitude at Birmingham Heath. The quantity of acetic acid used is less than 1-300th of the weight of the litharge, and the quantity of moisture found to be most advantageous is such as will just render the litharge sensibly damp to the touch. A purer and more economical source of carbonic acid than bark has been found in the combustion of coke, and powerful machinery has been applied to facilitate the process, by exposing new surfaces to the action of the gas. The result has been, that the process is completed in as many days of the ordinary one requires months, and the product is of a purer white, and in opacity or body, and all other respects, at least equal to the usual white lead of commerce.

One or two other facts deserve mention, which are not generally known. It is singular that if the protoxide of lead known as massicot, and the protoxide known as litharge, be exposed to a high temperature, approaching to a red heat, the massicot will rapidly absorb oxygen, and become the ordinary red lead of commerce; while the same process goes on exceedingly slow with the litharge, if at all; but, on the other hand, if massicot and litharge be moistened with dilute acetic acid, and exposed to carbonic acid, the litharge will be converted into carbonate before the massicot is much affected. Another fact is, that white lead and oil combine with so much energy, that if linseed oil be poured upon a large quantity of white lead, and the mass be left undisturbed for a few hours, the temperature will become so elevated, as to carbonize the oil, and render the whole perfectly black. It seems also not generally known, that white lead possesses the power of destroying the colouring matter of linseed oil. If sulphate of barytes be mixed with one portion of oil, and white lead with another portion, the latter will appear comparative white. If the two mixtures be allowed to remain for some days undisturbed, a quantity of oil will gradually rise to the surface of both. In the former, the supernatant oil will have undergone no change; in the latter, the oil will be nearly deprived of colour, and will have acquired the degree of rancidity termed by painters fat. The colouring matter has not combined, as might have been expected, with the white lead, for if this be dissolved by the agency of a weak acid, the disengaged oil will also be found to have been bleached. A large quantity of white lead is required to produce this effect, and the precipitated carbonate is less efficient than the white lead of commerce.

On Matias Bark. By Dr. Mackay.

Dr. Mackay read a communication upon a bark which he had lately received from South America, and stated to possess febrifuge qualities equal to those of the best Peruvian bark, for which it has been successfully substituted.

Dr. Mackay submitted to the inspection of the Section specimens of two different oils, obtained from the bark by distillation with water,

which, though existing in the same plant, and procured by the same process, present marked distinctions.

The one, being of lower specific gravity than the water, floated upon the surface of that which distilled along with it; while the other, being considerably heavier, sunk to the bottom of the receiver. Both, when fresh, were transparent and colourless; but in a few days they changed to a yellow colour, the heavier assuming a deeper tinge than the other.

In smell the oils differ perceptibly, that of the heavier being fatty and unpleasant, while the odour of the lighter is aromatic and agreeable. In taste they are equally acrid and disagreeable. The specific gravity of the lighter oil is 0·949, that of the heavier 1·028, both having been examined several days after their preparation. Upon exposing them to a temperature of 18° Fahr., no effect was produced upon the lighter oil; but in the heavier a great quantity of sparkling needle-shaped crystals were observed, which, however, speedily dissolved upon the phial being removed out of the freezing mixture. The mineral acids rapidly decompose them, converting both oils into fluids of a deep red colour.

In a chemical point of view the oils referred to are interesting, upon account of their presenting such marked distinctions in colour, smell, specific gravity, and the effect of cold upon them, although they are the produce of the same plant.

On an improved method of graduating Glass Tubes for Eudiometrical purposes. By CHARLES THORNTON COATHUPE.

The instrument employed by Mr. Coathupe for this purpose consists of a truly-bored cylindrical tube of iron, into which an iron piston is accurately fitted. Upon the rod of the piston a screw was cut with a good pair of dies, throughout its entire length. The rod is then filed into a triangular form, leaving a sufficiency of the threads of the screw at the rounded angles for an iron nut to traverse with security and freedom.

To the upper extremity of this iron cylinder a cap of the same metal is screwed, and into this cap is screwed an iron stop-cock: to the stop-cock is attached a glass measure, with a narrow lip, by means of an iron connecting socket.

Near the opposite extremity of the cylinder an iron diaphragm, of about a quarter of an inch in thickness, is inserted, and is fastened in its place by a side screw or pin, and through this diaphragm a triangular-shaped hole is made, through which the piston rod can slide easily up and down, but without lateral shake.

Below the diaphragm, and at the extremity of the cylinder, the nut is inserted, whose action propels or retracts the piston, without the possibility of the piston itself deviating from a right line.

This nut enters the cylinder to the depth of about half an inch, and around the entering part a deep V-shaped groove is turned, into which the pointed ends of three steel screws enter through the exterior of the

cylinder at equal distances, in such a manner that the nut can be revolved freely, but cannot be otherwise displaced.

From the entering part of the nut a projecting portion forms a shoulder, which is graduated into equal parts. (This projecting portion may be of any diameter greater than that of the cylinder.)

On the exterior of the cylinder an index is fixed, by means of which any number of revolutions of the nut, or any number of equal parts of a revolution, can be ascertained.

To prepare this instrument for use, the piston is to be retracted to its lowest position, and the cylinder is to be filled with mercury (without air bubbles) by pouring a sufficient quantity of this metal into the glass that is attached to the stop-cock, and turning the plug for its admission within the cylinder.

If, when the cylinder is full, and while some mercury still remains within the glass measure, we turn back the plug of the cock, we get the air-way of the plug filled with mercury; and by pouring off the superfluity, we have the instrument in a proper state to commence graduating any tube for laboratory purposes. Thus, if the tube to be graduated be about one third of an inch in diameter, if we open the communication between the cylinder and the measure and propel the piston by one whole turn of the nut, and then close the communication between the cylinder and the measure by turning the plug of the cock, we have within the measure a quantity of mercury, which, when poured into the tube to be graduated, will give a tolerably long space for the first division; and such similar spaces may be successively marked by repeating the process, until the whole tube be equally divided from end to end.

Notice of an Apparatus for determining the quantity of Carbonic Acid Gas in deteriorated atmospheres. By CHARLES THORNTON COATHUPE.

The apparatus consisted of a glass tube of about 24 inches in length, and having an internal diameter of about half an inch. It was terminated at each extremity by a brass cap, into each of which a brass stop-cock was firmly screwed. The glass tube was divided into 175 equal parts, by equal measures of mercury; and these divisions were numbered upon opposite sides of the tube in such a manner, that let either end be uppermost, the graduations might be instantly read. Every experiment could thus be tested by a double reading, by simply inverting the tube, and waiting a few seconds until the liquid employed for any examination had drained to its ultimate level.

The liquid reagent to be employed was stated to be either a clear saturated solution of quick lime in distilled water, or any aqueous solution of potassa, soda, or baryta.

The mode of using the tube for ordinary purposes was described as follows:

Fill the tube with the air to be examined, by any of the well-known means, and close the stop-cocks.

Pour a drop or two of the reagent that may be preferred into the

terminating orifice of either of the cocks, so as to fill the space between its extremity and the plug, and retain this liquid by the end of the fore-finger. Insert the extremity thus prepared into the vessel that contains the preferred reagent, and remove the finger. Turn the plug of the inserted stop-cock for the admission of the reagent. Apply the lips, or an exhausting syringe, to the orifice of the upper cock, and commence the process of exhaustion. Gently turn the plug of this upper cock during the exhausting process, and when the reagent has risen so as to occupy 30, 40, or more of the lower divisions of the tube, turn back the plug, and close the upper stop-cock. After a moment's pause, close the lower stop-cock also. Note the number of divisions unoccupied by the reagent, the tube being held upright.

Apply the finger to the lower orifice of the cock which has been immersed, in order to retain that portion of the reagent which will occupy the space below the plug. Agitate the tube with its contents. Immerse the same end of the tube again in the vessel containing the reagent, and remove the finger. Turn the plug of the lower cock, and watch the rise of the liquid within the tube until it has attained its fixed level. Then close the stop-cock, by turning back the plug, and replace the finger. Agitate well.

Repeat this simple process until the reagent ceases to absorb air from within the tube. Then suspend the tube by a loop of wire from a pin or nail fixed into some convenient corner of the wall, until the interior surface has become drained, and a permanent level established. Now open the upper cock, when a small quantity of air will rush into the tube to relieve the tension, and with it, generally, a single drop of the reagent that had occupied the small space within the bore of the upper cock beneath its plug.

The difference between the number of divisions, or parts, first observed to be unoccupied by the reagent, and the number of parts lastly observed to be unoccupied, will be the number of parts of carbonic acid gas that was contained in the number of parts of air first observed.

It will be evident that a correction for tension will be necessary to complete the process. This can be easily obtained; and a table once supplied, and entered in the laboratory note-book, will suffice for all experiments with this instrument.

Mr. Coathupe explained a process by which the operator may construct such a table for himself.

On a New Safety Lamp. By the Baron Eugène de Menil.

The peculiarity of this lamp consists in its open chimney, the principle of small apertures being employed only for the admission of air, on each side of the wick, through tubes capped by metallic gauze. The strong flint glass cylinder inclosing the flame is protected by a dozen small bars of tinned iron. The oil is kept in the lamp at a constant level. The chimney, which rises above the general body of the lamp, is contracted at the summit and covered (not closely) by an arched

piece of metal. A reflector is placed within the glass cylinder to direct the light.

In this lamp the flame is never extinguished: in an explosive atmosphere loud noises give indication of danger: it is of cheap construction, and economizes oil, but cannot be intrusted to the workmen who are engaged in drawing coals along the galleries of the mine.

It was stated that this lamp had been favourably reported on by M. Charles Combes, Ingénieur des Mines.

Notice of a Chemical Abacus. By Dr. D. B. Reid.

This instrument consisted of a frame of wood, across which wires were placed, and upon which beads were strung, as in the instrument which is employed by Chinese clerks, and is to be found in most museums. Each wire corresponds to a chemical element, and the beads to atoms, while the names of the elements are placed on the frame at the extremities of the wires.

Remarks on Gas-Lighting. By the Count du Valmerino.

GEOLOGY.

On the Formation of Local Museums. By William Sharp, *Esq., F.R.S., F.G.S., F.R.A.S., President of the Bradford Philosophical Society, &c., Yorkshire.*

The author, after deprecating the heterogeneous nature of the collections in local museums, proceeds to point out the plan recommended by himself, and adopted by the Bradford Society, in which it appears that the primary object is a collection of the natural objects of the district within fifteen miles of the town. The museum is intended to include geological specimens illustrative of the structure of the neighbourhood, and in reference to quarries, manufactures and agriculture; the vegetable productions, with a view to improvements in cultivation; the animals of the district; and facts relating to the meteorology, population, manufactures and general statistics.

On the Origin of the Tubular Cavities filled with Gravel and Sand, called "Sandpipes," in the Chalk near Norwich. By C. Lyell, *Esq., F.R.S., V.P.G.S., with Additional Facts by* J. B. Wigham, *Esq.*

The chalk near Norwich is covered with ferruginous gravel, sand and loam, occasionally containing crag shells. The surface of the

chalk beneath the gravel is very irregular; in some places tubular hollows, having the form of inverted cones and filled with sand and gravel, extend downwards into the chalk. They vary in width from a few inches to eight yards and upwards, and in depth from a few feet to more than sixty. Some are tortuous, but most of those at Eaton, near Norwich, are perpendicular. The materials filling the pipes agree precisely with those covering the chalk, with the exception, that in the pipes they are unstratified. The pebbles in the gravel consist of rounded flints and quartz; but no shells or pieces of chalk, or any calcareous substance, occur in the pipes. In general, coarse sand and pebbles occupy the central part of each pipe, while the bottom and sides are lined with a fine ferruginous clay, destitute of calcareous matter, but permeable by water. The chalk for a short distance around the sandpipes is moist and softened, and slightly discoloured by an intermixture of clay. Further from the pipes it is white and perfectly soluble in acids. Those pipes, whose diameter is less than a foot and a half, are often crossed by horizontal layers of flint nodules, which have remained *in situ*, while their chalky matrix has been removed. The author hence infers, that the pipes were formed by the corroding action of water containing acid. But it is clear that the tubes were not first excavated to their present size, and then filled with gravel, for in that case the nodules of flint derived from the chalk would have fallen to the bottom of the larger cavities,—but this never happens, the larger flints being always dispersed irregularly through the sand and gravel which fills the tubes. Mr. Lyell therefore infers that the excavation and filling of the tubes proceeded contemporaneously and gradually, and that thus the flint nodules, when removed from their matrix, subsided upon the sand and gravel which had previously sunk. This is further proved by the fact, that the horizontal strata of gravel are sometimes seen to bend down into the mouth of a pipe, and there become vertical. Mr. Lyell is of opinion, that some of the larger tubes (if not the smaller ones also) have been caused by springs charged with carbonic acid, rising through the chalk. The fine layer of clay, which coats the surface of the tubes, may have been deposited by the percolation of rain-water at a later period; and some of the finer particles, being carried into the chalk itself, would cause the discoloration of that rock near the pipes.

It was further stated by J. B. Wigham, Esq., in a letter to Mr. Lyell, that examples of slanting and tortuous sandpipes occur near Heigham. At Thorpe there is a pipe which penetrates both the chalk and the superincumbent crag, the whole being covered with the usual gravel. The clay lining is found throughout, and over this lining is a thin stratum with impressions of shells. Near Norwich many springs come up in the chalk, and sandpipes are always found near them*.

* For a fuller statement on this subject, with illustrations, see Phil. Mag., No. 96, p. 258, October 1839.

Description of a Section across the Silurian Rocks in Westmoreland, from the Shap Granite to Casterton Fell. By J. G. Marshall, *Esq., F.G.S.*

The object of this paper was to explain the order of superposition of a series of strata exhibited in Westmoreland, and to identify them, by means of their organic remains, with several members of the Silurian system as defined by Mr. Murchison. The general strike of these strata is from S.W. to N.E., and their prevailing dip towards the S.E., interrupted, however, by many faults and reversals of dip, which are described in detail. The following is the descending order of succession, deduced from the section here described.

1. Carboniferous limestone, which at Kirkby Lonsdale overlies conformably the old red sandstone, and on the E. of Casterton Fell reposes unconformably on the Blawith slate. In Kendal Fell it rests unconformably on the Benson Knott rock. 2. Old red sandstone breccia, forming cliffs on the E. side of the Lune. 3. Red or gray tilestones, with nodules of cornstone, occurring in the bed of the Lune from Beckfoot to Killington; these beds contain but few fossils. 4. The Benson Knott Rock, composing Benson Knott and two other anticlinal ridges on the N.W. of Kirkby Lonsdale, consists of a compact, gray, arenaceous slate with an irregular cleavage chiefly in the direction W.N.W. and E.S.E. It contains numerous fossils, which have been found by Mr. Sowerby to agree with those of the Upper Ludlow rock. 5. The Blawith slate, a hard, gray, siliceous slate, with a prevailing cleavage from N.N.E. to S.S.W. This rock is 4000 or 5000 feet thick, and at Blawith on Coniston Lake contains a thin bed of limestone with fossils. 6. The Coniston limestone, consisting of blue flagstone, black slate and blue limestone, the latter with numerous fossils. These beds are much compressed, altered and contorted where they abut upon the Shap granite; but are better displayed towards the S.W. in Sleddale, Kentmere and Coniston, the fossils from which place have been identified by Mr. Sowerby with those of the Caradoc sandstone. Mr. Marshall considers both the Blawith slate and the Coniston rocks to belong to the Lower Silurian series; and if the Benson Knott rock has been satisfactorily identified with the Upper Ludlow rock, it appears that the Middle Silurian series is wanting in the district here described.

The Coniston limestone is underlaid by the Cambrian system of Cumberland, which has been described by Professor Sedgwick.

On a Basaltic Dyke in the Vale of Eden. By J. A. Knipe, *Esq.*

The Vale of Eden, or plain of East Cumberland, lying at the base of the carboniferous limestone of the Pennine chain on the east, and the group of primary mountains of the lake district on the west, consists of

new red sandstone, much obscured by diluvial and alluvial deposits. Gypsiferous beds are exposed at Cotehill and other places. At Broadfield the subjacent carboniferous limestone is brought up and tilted at an angle of at least 40°. Dip south. Commencing at its most western point on the east side of the river Petterell at Petterell's Crook, about six miles south of Carlisle, a columnar mass of globular concentric balls of basalt is seen, which is again exposed on the Penrith road and on the summit of Great Barrock. Continuing this line the dyke is traced at Armathwaite, Combe's Peak, Stony Croft, Cringle Dyke, Renwick, Ravenswater, Hartside Fell, and disappears about half a mile south of Tynehead smelting-mill. The length of this dyke is twenty-two miles, its width twenty to thirty yards; and as its course almost coincides with that of the great Cleveland dyke, the author suggests that it is not improbable that they may be connected.

On the Geological Horizon of the Rocks of S. Devon and Cornwall, as regards that Section of the great Grauwacke Group comprised in the counties of Somerset, Devon and Cornwall. By the Rev. D. WILLIAMS, *F.G.S.*

The author first combats the opinion expressed by Mr. Weaver in the Philosophical Magazine, third series, vol. xv. p. 109, that the culm strata of Devon are unconformable to the trilobite slates which underlie them. He shows that the two examples of unconformity adduced by Mr. Weaver, at Rumson Lane and Muddle Bridge near Barnstaple, are due to local derangement of the strata, and that the general conformity of these two series of rocks is proved by numerous observations in other places. The connecting link between them is the "Coddon Hill Grit," containing elliptical masses of "Posidonia Limestone," and repeatedly alternating with the slate rocks below and the culm strata above, along the north borders of the central trough, and on the south margin abrupted through the plant-bearing beds and the overlying Cornish killas; its broad anticlinal line thus constituting the southern border, extending from Bos Castle on the sea to the greensand and chalk-flints of Haldon near Exeter. These Coddon Hill grits, Posidonia limestones and floriferous beds, after repeated alternations and mineral gradations, are eventually overlaid by the slates of Cornwall,—a fact, he states, which is well shown at St. Mellion and Pillaton on the south of Callington, and about Pentilly Castle on the west bank of the Tamar three miles below Calstock, where a good section of the St. Mellion and Pillaton rocks is exhibited. Mr. Williams refers the whole of the fossiliferous and other slates and limestones of S. Devon and Cornwall to this intermediate part of the series, which he considers to be somewhat older than the old red sandstone and carboniferous limestone of other parts of England,—a view in which he considers himself fortified by the mountain-limestone type of many of the fossils from the Plymouth limestone. The strata of this region are

thus shown to be much younger than has been commonly supposed, and the elvans or trap dikes must be of a still more recent date, as they are seen to produce alteration in the new red sandstone.

Note on the Organic Remains of the Limestones and Slates of South Devon. By R. A. C. Austen, *Esq., F.G.S.*

Bronn, in the conclusion to his *Lethæa Geognostica*, corrects his former opinion, that the limestone of Torquay and Bradley was the equivalent of the mountain limestone [Bergkalk]. Mr. Austen's object in the present communication, is to show to how great an extent the organic remains of the limestone of S. Devon are identical with those of the Eifel. He gives a list of twenty-seven species which are common to both districts. The three species, *Pleurodictyum problematicum*, *Cyrtia trapezoidalis*, and *Calceola sandalina*, are announced for the first time as British fossils; together with Brachiopoda, belonging to the sub-genus Trigonotreta. From a study of these fossils Mr. Austen concludes that the limestones of S. Devon are equivalent to those of the Rhine, and also to certain strata in the S. of Ireland and at St. Sauveur in Normandy. He considers the whole to be rather older than the carboniferous series, and alludes to the great difficulty of finding an appropriate designation for them.

On the Economy of Fuel. By Mr. Thomas Oram.

Dr. Buckland communicated the results of Mr. Oram's further experiments on his method of manufacturing fuel from the waste coal-dust, by which it appears a greater degree of heat is obtained (a very small quantity of smoke escaping, with but little residue in the form of ashes or clinkers,) than from the coal from which the dust was taken. A saving also of one third in bulk is effected, which circumstance especially adapts it for steam-navigation. The method by which the fuel is prepared is as follows: one ton of dust-coal, 200lbs. of river-mud, 40lbs. of coal-tar, 30lbs. of lime, 30 gallons of water, the whole mixed together, and pressed into the form of bricks.

On Remains of Mammalia in the Crag and London Clay of Suffolk. By C. Lyell, *Esq., F.R.S., V.P.G.S. &c.*

The teeth of several species of mammalia have been obtained by Mr. W. Colchester and the Rev. E. Moore from the crag at Newbourn in Suffolk. They have been referred by Mr. Owen to a species of leopard, a bear, and a small ruminant. They are all more or less broken, and were found in company with the teeth of fishes of the genus *Lamna*. It has not been positively ascertained whether these mammalian remains were imbedded in the crag itself, or in certain

fissures which are filled with detritus of a later date; but Mr. Lyell inclines strongly to the former opinion*.

Mr. Lyell then mentioned the discovery, by Mr. Colchester, of the tooth of an opossum in the London Clay at Kyson, near Woodbridge. It was found, along with the teeth of *Lamnæ*, in a bed of sand about ten feet thick, covered by seventeen feet of London clay, and the whole overlaid, at a short distance, by the red crag. Mr. Owen considers this tooth to belong to a species allied to the Virginian Opossum. Further search has since been made on this spot by Mr. Colchester, accompanied by Mr. Wood; and part of a jaw, with a molar tooth, has been found, which Mr. Owen has decidedly referred to an extinct Quadrumane allied to *Macacus*. (See Magazine of Natural History, new series, vol. iii. p. 446.) This is the first instance of the occurrence of quadrumanous mammals in deposits of the eocene period; and it is thus proved that this order of animals existed long anteriorly to the human race†.

On the Discovery of an Ichthyosaurus. By Mr. W. Marrat.

In this communication Mr. Marrat announced that his son, Mr. F. P. Marrat, had lately met with an ichthyosaurus in the lias limestone at Strensham, near Tewkesbury. The fore paddles were eleven inches long, and eight broad; the hind paddles seven inches long, and five broad. The ribs were forty-six in number, and the largest vertebræ rather more than two inches in diameter.

On Marine Shells found in Gravel near Worcester. By Jabez Allies, *Esq.*

Mr. Allies exhibited a series of the marine shells of existing species which have been found in the gravel near Worcester since the publication of Mr. Murchison's Silurian System, in which work their occurrence, in this locality, is first noticed. At Bromwich Hill, on the west of Worcester, rolled shells of *Turritella ungulina* and *Cardium edule* have been found beneath twelve feet of gravel at about fifty feet above the Severn. Bones and teeth of the elephant and rhinoceros occur in the same bed of gravel. At Kempsey, four miles S. of Worcester, the *Turbo littoreus* has been found (in addition to the shells enumerated by Mr. Murchison at p. 533 of his work) beneath about twelve feet of gravel, from fifteen to twenty feet above the Severn.

* For a full account of the position of these fossils with figures of the same, see Taylor's Annals of Natural History, No. 23. p. 186, Nov. 1839.

† For the details of this paper and illustrations of the organic remains, see papers by Mr. Lyell and Professor Owen, *ibid*, pp. 189, 191.

On the Topography of Ancient Tyre. By W. R. Wylde, *Esq.*

Queries respecting the Gravel in the neighbourhood of Birmingham. By H. E. Strickland, *Esq., F.G.S.*

The author commenced this paper by referring to the division of superficial gravel into *marine* and *fluviatile*, which he had found to prevail in the S. of Warwickshire and Worcestershire. (See Reports of the Sections, vol. vi. p. 61.) In the hope of ascertaining how far the same division would hold good in the neighbourhood of Birmingham, he proposed the following queries, to which, however, no answers were given.

1. Does the gravel near Birmingham ever contain chalk-flints, fragments of oolite, &c., which may indicate a southern origin, or is it purely of northern extraction?
2. Does it ever contain marine shells?
3. Are these shells of existing or extinct species?
4. Does it ever contain bones of land animals, or freshwater shells?
5. What are the circumstances of position, material, &c., of the gravel (if any) in which mammalian bones or lacustrine shells are found, and is it distinguishable in any respect from the gravel in which marine remains are found?
6. Are mammalian remains ever found in company with marine shells?

On Microscopic Vegetable Skeletons found in Peat near Gainsborough. By Mr. Binney, *of Manchester.*

Mr. Bowman read a paper on some skeletons of fossil vegetables found by Mr. Binney in the shape of a white impalpable powder, under a peat-bog near Gainsborough, occupying a stratum of four to six inches in thickness, and covering an area of several acres. It remained unchanged by sulphuric, hydrochloric, and nitric acids, and by heat, and was concluded to be pure silica in a state of extremely minute subdivision. On submitting it to the highest power of the compound microscope, it was found to consist of a mass of transparent squares and parallelograms of different relative proportions, whose edges were perfectly sharp and smooth, and the areas often traced with very delicate parallel lines. On comparing these with the forms of some existing *Confervæ* of the tribe *Diatomaceæ*, which are parasitical on other *Algæ* both marine and fresh water, but so minute as to be individually invisible to the naked eye, the resemblance was found to be so strong as to leave no doubt of their close alliance, if not perfect identity. To enable the Section to judge for themselves, Mr. Bowman exhibited highly magnified drawings of some living species from the works of Dr. Greville, and also of the powder, which fully bore out the conclusion he had arrived at. They are therefore the counterparts of the fossil infu-

soria of Ehrenberg, and occupy the same place in the vegetable kingdom as those do in the animal.

On the Carboniferous and Devonian Systems of Westphalia. By R. I. Murchison, *Esq. F.R.S., F.G.S., &c., and General Secretary of the British Association.*

The author states that having in company with Prof. Sedgwick examined the older rocks of N. Western Germany and Belgium, it is the intention of his friend and himself to lay before the Geological Society of London a general memoir (illustrated by numerous fossils) on the classification of these ancient deposits, showing a succession of the Carboniferous, Devonian (or old red,) and Silurian systems.

The present communication bears chiefly upon one point of this analysis, and is offered as a collateral proof of the geological position of the culm-bearing strata of Devonshire and Cornwall, as stated by Professor Sedgwick and Mr. Murchison to the British Association in the year 1836.

Transverse sections in descending order, from the productive coal-field of Westphalia on the N.N.W. to the older zoic rocks on the S.S.E. were explained; and one from Dortmund by Schelke to the neighbourhood of Linburgh and Iserlohn was specially adduced, in which the various strata are exposed in fine natural sections in the following descending order.

1. Coal shales, coal, &c. (productive coal-field.)

2. Millstone grit series, with many impressions of large plants, and occasional thin seams of coal.

3. Thinly laminated arenaceous sandstones and shales, containing many grasses and small plants.

4. Flat-bedded, black, bituminous limestone and shale, charged with *Posidoniæ* and *Goniatites*, and alternating with courses of flinty schist, the *kiesel-schiefer* of German geologists. This band, identical in all respects with the black or culm limestones of Devonshire described by Professor Sedgwick and the author, is proved to be of the age of the carboniferous or mountain limestone by containing at its western end near Ratingen numerous well-known fossils of that formation.

5. Devonian rocks:—the old red sandstone, consisting of psammites, schists, and limestones of great thickness containing many of those peculiar fossils of Devonshire which first led Mr. Lonsdale to suggest to Mr. Murchison (*vide* Geol. Proceedings, 1839–40) that they would prove to be of the age of the old red sandstone.

6. Silurian rocks, &c., which rise up into mountain masses from beneath the overlying deposits*.

* The author has corrected the abstract so as to make it agree with the views adopted by Professor Sedgwick and himself after their last visit to Westphalia, subsequent to the Birmingham Meeting.

The order and sequence of these strata are indicated and maintained along the lower edge of the range of the large coal-field of Westphalia, the beds successively rising to the surface at angles varying from 30° to 40° in perfect conformity, and showing throughout the clearest and most complete transition into each other. It is particularly to the group No. 3 that the author directs the attention of British geologists, because it is in all respects identical with the culm-bearing strata of North Devon and Cornwall, first described by Professor Sedgwick and himself as being a portion of a true coal-field, and not belonging to the grauwacke or older transition rocks, to which they had formerly been referred*. The Westphalian sections establish the geological position of the culm strata of Devon and Cornwall more clearly than had been done by any stratigraphical evidence in Great Britain, by presenting masses of mountain limestone and Devonian rocks rising out from beneath the culmiferous schists, and thus the precise age of the latter is demonstrated.

A General Outline of the Geology of Warwickshire, and a Notice of some new Organic Remains of Saurians and Sauroid Fishes belonging to the New Red Sandstone. By G. Lloyd, *M.D., F.G.S.*

Dr. Lloyd briefly described, with the assistance of a coloured map of the county, and a section extending N.W. and S.E. from Birmingham to Chesterton, the general distribution of the coal of North Warwickshire, the new red sandstone, the lias, and the oolitic outliers in the southern part of the county.

The coal-field, he observed, extends from Tamworth in a direction N.N.W. and S.S.E. to Griff, where it is bent in a line nearly due south, and terminates at Wyken; whence it is evident that this coal-field has a double axis. The coal strata are thrown up at a highly inclined angle, with a westerly and south-westerly dip, by the protrusion of thick masses, sometimes preserving their lines of beddings of quartz rock (altered Caradoc sandstone) and greenstone, on the eastern edge of the field. The quartz rock, formerly described as millstone grit, of Hartshill and Tuttlehill, is extensively quarried for roadstones, and for the manganese with which it abounds. No organic remains have yet been observed.

The coal-measures were probably elevated during the deposition of the lower new red sandstone, but anterior to that of the middle and upper members, as is shown by the undisturbed sandstones and marls of Attleborough and Marston. It is probable that this coal is connected with that of Charnwood Forest rather than with the great field of South Staffordshire, there being evidence of rapid thinning out on

* See Report of the British Association for 1836.

the eastern edge of the latter field. At Stockingford, near Nuneaton, the "upper coal measures" make their appearance, containing a thin freshwater limestone with coal plants and traces of galena.

The lower new red sandstone is but slightly represented in this county, the magnesian limestone and conglomerates nowhere observed. The northern and central portions of the county are occupied chiefly by the variegated sandstone of great thickness, and the variegated marl also fully developed in some localities, in others it is extremely reduced. The surface generally is much covered by northern drift and local gravel. There is great difficulty in determining the precise limits between the variegated sandstone and variegated marl, from the absence of the muschelkalk. A thin bed, not more than two or three feet in thickness, of highly calcareous, extremely hard, almost crystalline, white marl, but containing no organic remains, occurs at Garrison Hill on the Birmingham and London railway, but which, from its very limited extent and the absence of the characteristic remains, does not perhaps deserve the appellation of an humble representative of muschelkalk. In the variegated sandstone at Allesley near Coventry a part of the trunk of a large coniferous tree about two feet in diameter has been exposed, and eight or ten feet removed. The structure of the wood is identical with the driftwood which abounds in the gravel of the neighbourhood. The only animal remains yet observed in this part of the new red system is a broken jaw of a sauroid fish, containing fifteen teeth, found near Coventry. A considerable thickness of light-coloured sandstone, thick-bedded, with nodules of green marl occasionally interspersed, occurs at Warwick and Leamington, and extends, with occasional interruptions, to the north-east as far as Attleborough; some beds are very hard, containing much carbonate of lime. The saline springs of Leamington are in this sandstone, and small portions of rock-salt have been found in it. The sandstone is observed in some localities, as near Leamington, to pass gradually into the variegated marl; and it is asserted that it does not range uninterruptedly over large tracts, but occurs in wedge-shaped masses, thinning out in the strike and dip into the marls. The thickness of the variegated marl intervening between the sandstone at Leamington and the lias is less than 100 feet. In the variegated marl at Shrewley Common, about five miles west of Warwick, is a thin-bedded sandstone, hard, calcareous, white, sometimes veined and mottled with red, and has been exposed to the depth of thirty feet, including the interbedded red and green marl. This stone, which Mr. Murchison and Mr. Strickland consider is the true keuper sandstone, is, like the Warwick sandstone, of local appearance, though of frequent occurrence in the same parallel, and passes into the marl. Footsteps of a Batrachian (?) reptile were discovered by Mr. Strickland two years since in this stone. No organic remains have yet been found in it excepting *Posidonomya minuta*, (a shell common to the keuper and variegated sandstone,) and a few indistinct fragments of reptilian bones. Ripple and worm marks are frequently observed.

On the Organic Remains of the Warwick Sandstone.

The author stated that remains of animal forms recently found at Warwick and in the neighbourhood, apparently distinct from those of the new red system of the Continent hitherto published, had been submitted to Prof. Owen, who after minutely examining them had described them in detail under the names of

Platygnathus rugosus, certainly of reptile organization.
Dolicognathus Lloydii, *Dolicognathus varvicensis*, } probably sauroid fishes.

Crenated teeth of a large saurian, an episternal piece of *Phytosaurus*, and coprolites of various forms have been found in the same localities.

On Fossil Fishes from St. George's Colliery near Manchester. By Mr. Binney.

On the Foot-prints and Ripple-marks of the New Red Sandstone of Grinshill Hill, Shropshire. By O. Ward, *M.D.*

The stratum in which these impressions are found consists of a finely laminated buff-coloured flagstone, from five to ten yards thick, overlaid by two yards of a rubbly corroded red sandstone called "Fec," and underlaid by twenty yards of a buff-coloured massive building-stone, which rests upon a red sandstone of unknown thickness at the base of the hill. The ripple-marks are of three kinds: fine ones with sharp edges; wave-marks, more or less continuous elevations and depressions with smooth rounded surfaces; and little stream-marks. Foot-prints and rain-drops only found on the ripple- and wave-marks. The rain-marks are not always perpendicular to the general surface, but appear to have struck forcibly against the opposing face of a wave-mark, while they have glanced off from the sloping side; thus indicating the direction of the wind at the moment of formation. The foot-marks differ from those of the *Cheirotherium* in having only three toes, armed with long nails, directed forwards, not spreading out, and one hind toe on the same side as the longest fore toe, pointing backwards, and having a very long claw. No impression of the ball of the foot in this example; but in another there are three toes, and a depression for the ball not unlike that of a dog. The foot-prints and ripple-marks were made upon a surface alternately wet and dry, probably on the shore of a sea or tidal river, dry land being near, of which this hill formed the beach. The strata take the slope of the hill northwards, and the

streamlets flowed in the same direction. From the gradual passage of flag-stone into the massive building-stone, it is argued that the former has never been under much pressure in the upper part, and therefore has always maintained nearly the same distance from the surface that it now has.

On the Action of Acidulated Waters on the surface of the Chalk near Gravesend. By the Rev. W. BUCKLAND, *D.D., F.R.S., F.G.S., &c.*

The author first adverted to the frequency of caves in connexion with fissures in the limestone rocks of all countries; and considered the enlargement of many fissures into caverns to be due to the corroding action of acidulated vapours and waters. He cited a remarkable example of a very lofty dome-shaped cavern at Pantalica, near Syracuse, which he considered to have been produced in this manner.

He also attributed to the corroding action of water charged with carbonic acid, the origin of the deep irregular furrows, pits, and gullies which are so frequently found on the surface of the chalk, and are usually filled with gravel mixed with clay and sand; and cited examples of excavations of this kind in deep sections of the chalk hills by the road-side between Beaconsfield and High Wycombe, and at the top of the hill immediately east of Henley in Oxfordshire. He refers to the epoch of the plastic clay formation the gravel and clay filling these cavities near Beaconsfield. Here remains of tortoises were found in the clay by the late Lord Grenville.

About ten years ago a remarkable opportunity occurred at Gravesend of seeing a large surface of chalk stripped of its covering of sand and gravel, and exposing the condition it had acquired before the arrival of this covering, which was removed in order to get access to the chalk. Before the deposition of this sand, in which were many bones of deer and other mammals, a shallow bason, about twenty-five feet deep, and extending over a quarter of an acre, appears to have been formed on the surface of the chalk; the entire bottom of this bason presented a surface covered with irregular bowl-shaped cavities, separated from one another by intermediate rounded hillocks and ridges, resembling the little pits and ridges that appear on the surface of a piece of limestone that has been steeped in acid. Most of these cavities were about three feet in diameter, and their depth varying from one to two feet. Both the elevated and depressed portions of the chalk had the same smooth and somewhat glossy surface that compact limestone presents after corrosion by an acid.

Dr. Buckland referred all these phænomena to the action of acidulated waters, and was of opinion that the frequent volcanic eruptions during the tertiary period might have impregnated the sea in certain regions with carbonic acid, which would account for the extensive corrosion and destruction which the chalk underwent at that epoch, when

the gravel-beds of the eocene period were supplied with flints, set free by the solution of the chalk in which they had been formed, and subsequently rounded by the action of water. He considered also that the carbonic acid contained in rain-water has produced in more recent periods, and still continues to produce similar effects in corroding and forming cavities on the surface of chalk beneath permeable beds of gravel or sand.

Exact and beautiful drawings of these appearances at Gravesend were made at the time of their discovery by Mr. Thomas Webster, for Dr. Buckland, and were exhibited at the Section to illustrate his observations.

On an œconomical Use of the Granitic Sandstone of North Staffordshire. By ROBERT GARNER, *Esq., F.L.S.*

This paper announces the recent discovery of a valuable property in a substance hitherto esteemed worthless. Previously to the last three or four years it has been the practice to import chalk-flints at a great expense into North Staffordshire, for the use of the earthenware manufacture. It has been lately found that the millstone-grit of the Pottery district will answer the same purpose, and it is now quarried to the extent of many hundred tons annually. It is found to be a perfect substitute for flint, with the advantage of not requiring calcination previously to being ground. The material for the pottery is compounded of about equal parts of millstone-grit, Dorsetshire and Cornwall clay, and the ware produced is found to possess the qualities of whiteness and compactness in a high degree. The best specimens of millstone-grit are those which contain about three parts of silica and one of alumina, and which are free from iron or sulphate of barytes.

On the rapid Changes which take place at the Entrance of the river Mersey, and the means adopted for establishing an easy access to Vessels resorting thereto. By JOS. BROOKS YATES, *Esq.*

The author commences this paper with an historical sketch of the changes which have taken place at various times in the æstuary of the Mersey. It appears that at some distant period this æstuary was principally occupied by peat mosses and forests, vestiges of which are still found beneath the sands on the coasts both of Cheshire and Lancashire. These mosses appear to have been submerged by an irruption of the sea, which has encroached so much on the land within the last few years as to require the erection of a sea-wall to protect the Leasow lighthouse.

The changes in the submarine sand-banks are shown by a comparison of ancient with modern charts. From a survey made in 1687 by Capt. Grenville Collins, it appears that large ships were obliged to unload

part of their cargo before they could come up to Liverpool, which at that time was inferior to Chester in importance. The entrance has since become deeper, and a channel called "Helbre Swash" has been formed across the "Hyle sand," which in 1687 was dry at high water of the neap tides. Many similar changes are shown to have taken place, but the most important is one which is now in progress by human agency. This is a new channel, about one third of a mile wide and three quarters of a mile long, which has already been deepened upwards of four feet, and will shortly be thrown open to shipping under the name of the Victoria channel. The deepening is effected by dragging a large iron harrow, invented by Lieut. Lord, over the sand-bank by means of a steam-vessel. This process is continued daily during the ebb tide, and the sand and mud thus loosened from the bottom is carried out to sea by the current. The complete success of this invention recommends it to the notice of persons connected with other harbours.

On Peat Bogs. By G. H. Adams, *M.D.*

Prof. Shepard, of Yale College, exhibited a collection of organic remains from the limestones of North America, numbered; many of the specimens corresponding to the numbers towards one end of the series, belonging certainly to species described in Mr. Murchison's work on the Silurian System; many of those toward the other end as certainly identical with species figured by Professor Phillips in his work on the Geology of Yorkshire.

ZOOLOGY AND BOTANY.

On the Formation of Woody Tissue. By Mr. Edwin Lankester.

The tissues of plants, for the sake of convenience, are divided into five, the origin of which may be all traced to the simple cell. *How* they are formed, is an undecided question, more especially with regard to woody tissue. Du Petit Thouars supposes that woody fibre is formed by the buds and leaves, and sent down by them between the bark and wood, where they are nourished by the cambium. Others suppose that it is formed from the wood or bark. The most prominent features of woody tissue are its length, and hardness from the deposit of secretions in its interior. These points, however, do not constitute an essential difference between woody and cellular tissue, as we find the latter *lengthened* in the form now called Pitted tissue, or Vasiform tissue, and *hardened* as in the endocarps of *Amygdaleæ*, &c.

If in the term *woody tissue* all lengthened, hardened tissue be in-

cluded, then we find it present in many instances where neither buds, leaves, or bark, can be said to exist, as in Cryptogamic plants, especially various species of *Boleti.* It is also found in many parts of Phanerogamous plants, as the pericarps of the cocoa, beech, and other plants which in those parts are destitute of leaves. The author had also found woody tissue in abundance in the leafless Monotropa, and in many species of *Cactaceæ.*

Another objection to the theory of Du Petit Thouars is found in the fact of wounds of trees filling up at the lower as well as the upper lip. In trees that had been felled, the author had observed the production of fibrous tissue independent of leaves or buds, (specimens of this were exhibited to the Section).

The author then detailed some experiments he had made by ringing beech trees in the spring of the year. When cut down in August, a cellular and woody formation appeared both in the upper and lower lips of the wounds, the woody tissue having been formed subsequent to the ringing.

The last occurrence to which the author directed attention, and which could not be explained by this theory, was the existence of knobs of wood in the bark of beech and other trees. These excrescences are of all sizes, and when first formed, are cellular; they gradually harden, and at last present layers of contorted woody fibre. They have a regular bark of their own, filled with sap during the spring, and present, when cut, concentric circles of woody tissue representing their yearly growth. Many of them put forth buds, and some few of them leaves, but by far the greater number have neither buds nor leaves. Sometimes several are found together in a mass (especially in the elm and acacia), each nucleus having a separate series of concentric layers surrounding it. Although, from rapid growth, the compound knobs are found in contact with the wood of the tree, the single knobs are seldom found in this position. These knobs have been called by Dutrochet *embryo buds.* The conclusions which the author advanced, from his present knowledge of the facts, were,

1st. That the requisites for the formation of wood are, 1. a living tissue developing elongated fibres; 2. a tissue forming and depositing secreted matter; and 3. exposure to the influence of external stimuli.

2nd. That the secreted matters are more easily brought under the influence of external stimuli in the younger tissues; hence the importance of leaves.

3rd. That neither bark nor leaves are essential to the formation of woody fibre.

Notice of Zoological Researches in Orkney and Shetland during the month of June 1839. *By* EDWARD FORBES *and* JOHN GOODSIR, *Esquires.*

During their short excursion, the authors directed their attention almost exclusively to the invertebrate animals. Of mollusca, they

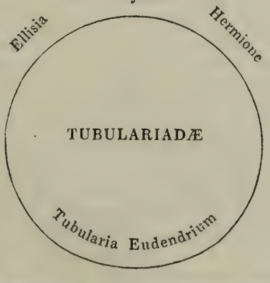
found five species of the genus Eolida. Of these, four were undescribed, the other being *Eolida papillosa* of authors, which abounded in Shetland, under stones at half tide, whither it appeared to resort for the purpose of spawning. The four new species are named by the authors *E. Zetlandica*, *E. coronata*, *E. foliata*, *E. minima*; the two last were obtained by dredging in seven fathoms' water. They found no Eolidæ in Orkney.

Of the genus Euplocamus they found one species, allied to the *Euplocamus pulcher* (*Triopa clavigera* of Johnston), but differing from that species, in having its branchiæ, both lateral and dorsal, tipped with yellow. Its back is white, spotted with yellow. Of the genus Doris they found two species, one the *Doris pilosa* of Müller, and the other *Doris bervicensis* of Johnston.

They found one new testaceous mollusk, a species of Velutina, which the authors have named *Velutina elongata*. Ascidiæ abound in the north; the more common species is the *Ascidia intestinalis*. Along with it they found three species, which, there is reason to believe, are undescribed. The authors propose to name them *Ascidia echinata*, *Ascidia rugosa*, *Ascidia rubens*, the two latter are from Orkney. Of Annelides the authors found great numbers; such as they collected they intend submitting to Dr. Johnston, as the highest British authority on that class. They observed Planariæ in great numbers: among others was the beautiful *Planaria atomata* of Müller, not before recorded as British. Among the Radiate animals they were especially successful. The genus Holothuria is conspicuous in Shetland; among them is an enormous species, which the authors name *Holothuria grandis*. This splendid animal is fully two feet long, when extended; it is of a deep purple colour; it has ten triangular frondose tentacula, purple, spotted with white; its body, between the rows of suckers, is almost smooth. The other new species of this genus observed were—*Holothuria fucicola*, Shetland, *Holothuria brevis*, *Holothuria fusiformis*, *Holothuria lactea*, *Holothuria pellucida* of Müller. Along with Holothuriæ the authors dredged the *Priapulus caudatus*, and *Sipunculus strombi*. They found no Holothuriæ or Priapuli in Orkney.

Of the sea-urchins they found only the *Echinus esculentus*, and a form which appeared to be the *Echinus neglectus* of Lamarck. The star-fishes observed were *Asterias aranciaca*, and an allied form, probably the *Asterias bispinosa* of Otto, *Stelloniæ rubens* and *violacea*, *Luidia fragilissima*, *Solaster papposa*, *Ophiura albida* and *texturata*, *Ophiocoma bellis*, *granulata*, *rosula*, *neglecta*, and a new species. The Medusæ doubtless abound in these islands in August, their proper season; but when the authors of the paper were there, they observed only *Cyanæa capillata*, *Medusa aurita*, a new Dianæa, a new Oceanea, a new Ciliograde, of the genus Alcynöe of Rang, and a minute animal, the type of a new genus among the Acalephæ. Sponges of the genera Grantia and Holochondria abound in Shetland. From deep water the authors obtained several specimens of *Tethya cranium*, and kept them alive in salt water, but could observe none of the contractions stated to have been seen in that species by some of the French naturalists.

The most beautiful contribution to the British Fauna from the Orkneys, is a zoophyte of the family Tubulariadæ, the largest known form of its tribe. This beautiful animal is about four inches long, and its stem half an inch in diameter. This stem is rounded, solid, flexible, moving at the will of the animal, and somewhat contractile. It is translucent, of a pinkish-white colour, lineated with brown longitudinal lines, arranged in pairs. When young, the stem is shorter, and is inclosed in a delicate, brown, corneous tube, which becomes deciduous as the animal grows larger. The lower part of the stem is broader than the upper, and roots in sand by means of a fusiform termination, sending out corneous filamentous roots. At the upper extremity the stem becomes suddenly contracted, and the lines terminate; it then expands into an ovate head, terminating in a long, pyramidal, pink trunk, at the end of which is the mouth. Round the thickest part of the head is placed a row of about forty long, white, uncontractile tentacula, which wave about in all directions, and are not ciliated. Immediately above the circle of tentacula is a circle of about twenty-five ramified orange processes, probably ovarian, having no voluntary motion. Above this the trunk is covered with numerous white tentacula, very much shorter than the outer circle. Within this head is a simple digestive cavity, not extending down so far as the large tentacula. Every other part of the animal is solid, and no part is ciliated. Beautiful and delicate as these animals appear, they are very tenacious of life. They were dredged in considerable numbers, on a sandy bottom, in about ten fathoms' water, at Stromness, Orkney. The position of this animal is between Tubularia and Coryne, on the relations of which genera its discovery throws much light, as well as on the polypes in general. The authors propose to consecrate the genus to that great British zoophytist, Ellis, calling it Ellisia, and giving the species the appropriate name of *Flos maris*, as it may well be regarded, from its extreme grace and beauty, as the flower of the British seas. The relations of Ellisia to Tubularia may be exhibited by the following diagram :—



NOTE.—The Ellisia has since proved identical with the *Corymorpha nutana* of Sars.

Coryne—Tentacula scattered, of one kind; no tube.
Hermione—Tentacula scattered, of one kind; tube.
Eudendrium—Tentacula of one sort, regular; branched tube.
Tubularia—Tentacula of two sorts, regular; simple tube.
Ellisia—Tentacula of two sorts, regular; deciduous tube.

Mr. W. R. Wilde exhibited three drawings of a Peruvian mummy, showing its different states of development.

Mr. Lankester made some observations on the preparation of fishes for museums. He exhibited several specimens, which, after having taken away one side, he had allowed to dry, and assume their natural state, and then placed them on paper. The process consisted in drying the fish, then taking away their soft parts, then drying the skins, keeping them in shape by pieces of stick and cork, and, finally, varnishing them with mastic varnish, by which they become stiffened, and their colours preserved.

On the Follicular Stage of Dentition in the Ruminants, with some Remarks on that Process in the other Orders of Mammalia. By JOHN GOODSIR, *Esq.*

Mr. Goodsir commenced by stating, that since the last Meeting he had detected the follicular stage of dentition in the pig, rabbit, cow, and sheep, but that he had not had an opportunity of examining it in those animals in which observations would have been most valuable. He had been able to verify, what at that time he stated as probable, viz. that all the permanent teeth, with the exception of the first molar, which does not succeed a milk tooth, are developed from the internal surface of cavities of reserve, and that the depending folds of the sacs of composite teeth are formed by the lips of the follicles advancing inwards, after closure of the latter. He then described the progress of development of the pulps and sacs of the teeth in the cow and sheep, from their first appearance, as minute as possible, on the full surface of the membrane of the mouth, or on the internal surface of the cavities of reserve, till they have acquired their ultimate configuration. In the course of this description he announced the fact, that at an early period of the embryonic life of these animals, they possess the germs of canine and superior incisive teeth; the former existing as developed organs in two or three genera only of ruminants, the latter being found in the aberrant family of camels. Mr. Goodsir stated, that these germs presented themselves under the form of slight dimples in the primitive groove, and that after the closure of the latter, they remain for a short time opaque nodules imbedded in the gum, in the course of the line of adhesion. The existence of germs of canines and superior incisors in the cow and sheep is highly interesting, as it shows how general the law of unity of type is within certain limits. Geoffroy St. Hilaire was the first to announce the existence of tooth germs

in the fœtus of the *Balæna Mysticetus*, a fact which has been verified by Dr. and Mr. Frederic Knox, in whose museum there is a preparation exhibiting the germs under the form of sacs and pulps. Although the germs never arrive at this stage of perfection in the cow and sheep, they are yet distinct enough to indicate their existence; and the author of this paper has no doubt, that when the embryos of other partially or wholly edentulous mammals have been examined, similar results will be obtained. The author then proceeded to state, that the peculiar manner in which the sac of a ruminant molar, and probably of every other composite tooth, is formed, may be best seen in longitudinal or transverse sections of the sac and pulp of the fourth permanent molar of the sheep or cow. The internal surface of the cavity of reserve is seen to end in a fold or folds; when these meet, they begin to curve towards the papilla, and to enter parallel to one another the cavity or notch which is simultaneously forming in the latter. As soon as the edges of the folds meet, the granular matter denominated enamel pulp by Hunter (the formation of which was described by Mr. Goodsir in the human embryo, at the last Meeting of the Association,) begins to be deposited, cementing together the opposing folds, sealing up the new sac, separating it from the rest of the cavity of reserve, filling up the space existing between the pulp and sac, and ultimately assisting in the formation of the depending folds of the latter. The author then referred to the distinction which must be drawn between those permanent teeth, which are developed from the primitive, and those which are developed from the secondary groove; and stated that he had been in the habit of dividing the teeth of these animals, the dentition of which he had examined, into three classes; viz. 1. Milk or primitive teeth, developed in a primitive groove, and deciduous. 2. Transition teeth, developed in a primitive groove, but permanent. 3. Secondary teeth, developed in a secondary groove, and permanent. Mr. Goodsir expressed a hope that other anatomists would verify and extend this line of research, as the results appeared to him not only confirmatory of certain great general laws of organization, but as leading, in his opinion, by the only legitimate path, to the determination of the organic system to which the teeth belong, (a subject exciting great interest at present,) and as it might enable us, in investigating the relations of dental tissue to true bone, to avoid the error of confounding what there appears to be a tendency to do, analogy with affinity. The paper concluded with a recapitulation of the principal facts contained in it. 1. In all the mammalia examined, the follicular stage of dentition was observed. 2. The pulps and sacs of all the permanent teeth of the cow and sheep, with the exception of the fourth molar, are formed from the minor surfaces of cavities of reserve. 3. The depending folds of the sacs of composite teeth are formed by the folding in of the edges of the follicle towards the base of the contained pulp, the granular body assisting in the formation of these folds. 4. The cow and sheep (and probably all the other ruminants,) possess the germs of canines and superior incisives at an early period of their embryonic existence.

On the Preparation of Fish. By Mr. WILDE.

In this mode of preparation an incision is made through the scales to the muscles, commencing about where the operculum joins the cranium, and continuing it parallel with the dorsal outline to the centre of the tail. A similar cut is made from above the pectoral fin, till it also reaches the centre of the tail; by this means somewhat less than a third of the side is included between the lines. The fish is kept steady on a smooth board, adhering to it by the natural gluten, water being poured over it from time to time, so as never to allow the scales to dry. The skin is then dissected back as far as the dorsal margin, where it meets the bony rays which support the fins. These are cut across, as close to the skin as possible, with a strong pair of scissors or a cutting forceps. A similar process is used towards the abdomen, taking care to keep as close to the fascia to which the scales are attached as possible. The first vertebra is then divided from the cranium, and the skinning process continued by lifting up the body and leaving the skin adherent to the board, from which it should never be removed, if possible, till the dissection is completed. Difficulty will be experienced towards the tail, where the muscles become more tendinous, and are attached to the subcutaneous fascia. The rays of the caudal fin are then divided from the vertebra, and the body removed entire. The gills are next taken out, and the flesh of the cheeks and any remaining portion about the head or thorax. It is as well, perhaps, to leave in the scapulæ, or a large portion of them. An opening is made into the side of the cranium, where it will be found very thin, and the brain taken out. The eye is completely removed on the reverse side; a hook, passed down through the orbit, transfixes the back of the sclerotic of the other eye, in which an opening is made; the finger then pressed on the cornea in front will squeeze out the lens and humours, retaining the iris perfect in its place, and the author has lately succeeded in retaining the gills, if necessary; the tongue is left in, and the fish is then cleansed from all impurities, taking care not to stretch the skin nor to injure the scales. It is next dried, and either well anointed with arsenical preparation or the spirituous solution of corrosive sublimate. The eye is filled with cotton from the opening in the back; care being taken to keep the iris in its natural position. The cranium is also stuffed; and flakes of tow, cotton, or any material of light description, laid along the body till a sufficiency to give the form of the animal is put in. The reflected edges are then returned; the fish is removed from the board, and placed with the front up; the tail and fins, expanded, are pinned down in their natural position, on cards, supported by little bits of cork; the fish is given its proper shape, and the inequalities on its surface smoothened off with a soft brush; it is then set to dry in a current of cool air, with little sun, and should be watched to see that it dries equally, and that no part of the skin shrinks more than another. If it should, a brush, wetted in cold water, touched upon the part, will restore it. It should be varnished the moment it is sufficiently dry, and the cards, &c., removed from the

fins, which will now retain their natural position. Common copal varnish, diluted with turpentine, is recommended as the best. The cornea now becomes hard, transparent, and continuous with the surrounding skin: the wadding may be removed through the back of the sclerotic, and a bit of foil introduced in its place, of the colour originally possessed by the animal, in many of which we know the tapetum is very brilliant. The author noticed the difficulties which attach to this process in fishes with small scales, and described the methods by which they were overcome.

Mr. R. Patterson exhibited drawings made from living specimens of a species of Ciliograde, taken in July last at Bangor, County Down. Its occurrence on the Irish coast had first been announced in a note appended to his paper on the *Cydippe pomiformis*, published in the Transactions of the Irish Academy. This animal he had referred, though with some doubt, to the genus Bolina, of Mertens, and named it provisionally *Bolina Hibernica.* He mentioned some particulars relative to its appearance, movements, frangibility, and luminosity.

On the Ciliograda of the British Seas. By Edward Forbes, *and* John Goodsir, *Esqrs.*

The Ciliograda of the British seas belong to three genera—*Cydippe*, Alcynoë, and Beroë. The genera and species may be summed up as follows:—

Ciliograda.

Genus I. Cydippe, Esch.—Animal provided with filamentary appendages, but without natatory lobes or oral tentacula.

Genus II. Alcinoë, Rang.—Animal not provided with filamentary appendages, but furnished with natatory lobes and oral tentacula.

Genus III. Beroë, Linn.—Animal unprovided with filamentary appendages, natatory lobes, or tentacula.

I. Cydippe, Eschscholtz. (Pleurobrachia, Fleming.)

1. *Cydippe pileus* (Linnæus).—Rows of cilia, 19 or 20, on the summits of the lobes. Filamentary appendages white. St. Andrew's. Mouth of Thames. (Dr. Grant.)

2. *Cydippe Flemingii* (Forbes), (a *Beroë ovatus*, Fleming?). Rows of cilia 36, on the summits of the lobes. Filamentary appendages white. St. Andrew's.

3. *Cydippe lagena* (Forbes).—Rows of cilia about 25, placed in the furrows of the lobes. Filaments white. Coast of Ireland.

4. *Cydippe pomiformis* (Patterson).—Rows of cilia about 20. Filaments rufous. Coast of Ireland and Mouth of Forth.

II. Alcinoë, Rang.

2. *Alcinoë rotunda* (Forbes and Goodsir).—Ovate, rounded, crystalline; tentacula rounded at their extremities. Natatory lobes forming half the animal. Kirkwall Bay, Orkney.

2. *Alcinoë Smithii* (Forbes).—Elongato-pyriform, subcompressed, crystalline. Natatory lobes not more than a third of the whole length of the animal. Tentacula acute. Near Ailsa Craig and Irish coast.

III. Beroë, Linnæus.

1. *Beroë cucumis* (Otho Fabricius).—No spots on external surface, internal dotted with red points, ciliiferous ridges red.

2. *Beroë fulgens* (Macartney).

On some new Species of Entozoa, discovered by Dr. Bellingham.

On the Acceleration of the Growth of Wheat. By Geo. Webb Hall, Esq.

The object of Mr. Hall's communication was, to call the attention of the Meeting to a statement of facts connected with the acceleration of the growth of wheat, and a consequent diminution of the period required for its occupation of the ground, and to exhibit the results of the proceeding, and the benefit deducible therefrom. The ordinary period of growth allotted to the wheat plant may be taken from the middle of October to the middle of August—a period of ten months—twelve, or even thirteen, being not uncommon; while for the ordinary winter wheat, from December to August may be taken as the shortest period of growth: close observation of the progress of the plant, under different circumstances, and a peculiar selection of seed from warm soils, have reduced this period to nearly five months. An abundant crop of wheat, which was sown on the 2nd of March, was ready for the sickle on the 15th of August following. This is not a solitary case, nor is it the result of a peculiar season. In the year 1835, wheat sown on the 5th of March was reaped on the 12th of August; and on a previous occasion, wheat sown on the 9th of March was reaped on the 11th of August, the produce being forty bushels per acre. A deep tenacious soil is most congenial to the growth of wheat; such soils, however, form a very minute portion of the land of England; to the lighter and more siliceous soils Mr. Hall's observations apply. When wheat is placed upon the lighter soils, its growth and security are alike promoted by artificial pressure and compacting of such soils, which also, by the addition of manure, acquire a warm and stimulating character; but they as assuredly become quickly exhausted, and therefore the acceleration of the growth and ripening of the plants committed to a light soil, and a diminution of the time required for perfecting its crops, is congenial to its character, and tends to economize and prolong its productive powers. Mr. Hall wished to direct the attention of botanists to the practicability of so adapting the seed to the soil, and regulating the time of sowing, that an early ripe crop should be always obtained, and the accidents be avoided on a large proportion of our soils to which a growing crop is exposed in the depth of winter.

Notice of an Experiment on the Growth of Silk at Nottingham in 1839. By WILLIAM FELKIN, ESQ.

A large sample of yellow and pure white cocoons, forming a portion of the results of this attempt at raising silk in England, was placed before the Section of Natural History, upon the twigs where they had been spun by the silk worms,—the French and Italian mode of management being, so far as possible, adopted throughout the entire course of the experiment. Bertizen produced equally good cocoons somewhere near London, in 1790, but beyond his presenting the silk reeled from them to the Society of Arts, and receiving their premium, only few particulars of his experiment are known. In the present instance, the food supplied to the worms spinning the white silk (owing to the sudden and continued check to vegetation by severe east winds and frosts throughout May 1839,) was lettuce leaves during the first three weeks after hatching, afterwards they were fed entirely upon mulberry leaves. Those spinning yellow silk were hatched fourteen days later, and were fed from the beginning upon mulberry leaves. Of those fed partly upon lettuce, 7-8ths died; on the contrary, the greatest loss in those fed altogether upon mulberry was from 30 to 40 per cent. The average of loss upon the continent of Europe is from 35 to 60 per cent., the latter being the usual loss under the management of the peasants. That division of these yellow ones which spun first, and which were most healthy, experienced a loss of only 10 to 20 per cent. The loss in China, owing to their superior skill and care, is often not more than 1 per cent. of those hatched. The hatching in question was of eggs procured from Italy; and this, as well as all the subsequent processes of feeding and spinning, took place in a warehouse in the centre of the town of Nottingham, amidst the usual noise, dust, and activity of a wholesale business in cotton goods, where the air must have been in some degree tainted by the oily matters used in their fabrication. The weather for three weeks from the 14th of May was dry, but piercingly cold; then, after an interval of two weeks of fine weather, there was constant and most unusual humidity, so that it was almost impossible to refresh the air of the apartment, or avoid giving the food in a damp and heated state; especially as, from the number of worms (about 10,000), much difficulty was experienced in obtaining mulberry leaves in sufficient quantity for their use, these having to be collected from places in some cases 50 or 80 miles distant. Such was the continuance of rain in July, that the largest flood occurred ever remembered at that season of the year. To suit our variable climate the temperature of the room was from the first kept low, varying from 70 to 55 degrees. Altogether, the circumstances under which this experiment was made were very unpropitious.

In addition to the usual diseases which Mr. Felkin had observed, when formerly investigating the French and Italian management of silk worms, one occurred immediately after a violent storm of thunder, lightning and rain, which was quite new to him. The worms were nearly ready to spin, and those affected were found dead or dying; the

wrinkled part of the body to which the head is attached, quite black, and the skin of the neck thickened and tough like leather. Again, as the heavy and damp weather continued, a disease occurred, affecting the two front and two hind rings of the body, by producing an unnatural and evidently very painful contraction of the parts, and a corresponding enlargement of the four middle rings, which usually ended in the sac bursting, and of course destroying the insect. It may be remarked, that the pulsation visible along the back of the worms was, in the case of those fed upon lettuce, reduced to 20 and 25 beats a minute: in order to ascertain whether this arose from the food, some were fed with mulberry, while the rest were continued upon lettuce. The former exhibited a daily increase in the pulsation of about 5 beats; until, at the end of three or four days, the muscular expansion and contraction along the back was of the usual quickness, *i.e.* 40 to 45 beats a minute; which was what those fed entirely on mulberry leaves invariably exhibited.

The time of spinning in Italy is usually six weeks after hatching. In Nottingham the earliest did not spin until eight weeks after hatching; but such as were originally fed upon lettuce did not spin until those fed entirely on mulberry had finished their cocoons; the lives of the former were therefore protracted full three weeks beyond the latter.

The cocoons being placed in contrast with those (also on the table of the Section) of this year's growth, just received from the Milanese, presented but slight inferiority in size, weight, or compact formation. Of those grown in Nottingham, it took an average of 300 to weigh a pound, while of the best French or Italian, it takes at least 250. The English acclimated cocoons weighing, when dry, 1 to $1\frac{1}{2}$ grains, fed upon lettuce and mulberry; those of Bengal, 1 to $2\frac{1}{2}$ grains, fed on Indian mulberry; Italian, 3 to 6 grains, fed on white mulberry; Nottingham, $2\frac{1}{2}$ to 5 grains, fed on black mulberry; New Jersey, U. S., two crops a-year, 5 grains; and New Jersey, Mammoth, 6 to 8 grains, (the last two fed on *Morus multicaulis*,) were exhibited to the Section.

The chief object in view, in bestowing the time and labour necessary to bring about the results which establish the interesting and important fact, that silk of the best quality could thus be grown in England, was to show how the produce of this article might be greatly improved in quality, and indefinitely increased in quantity, *in Hindoostan.* There, labour is cheaper than anywhere besides; and land unoccupied and waste, but perfectly suitable for the mulberry, is plentiful; so that, by not confining the cultivation of silk to the marshy Delta of the Ganges, as at present, but introducing into the more elevated and even mountainous parts of Hindoostan, &c., the superior kinds of silk worms and mulberry trees so long grown in the south of Europe, and recently cultivated in the United States of North America, raw silk might be supplied from India at half its present cost—a cost increased by the demand greatly exceeding the supply, so as to have compelled us to pay four instead of three millions sterling a-year,

during the last four years, for the same weight of material, and thus greatly to limit the extent, and even to risk the safety of the silk manufacture itself.

Some Observations on Whales, in connexion with the account of the Remains of a Whale recently discovered at Durham. By GEORGE T. FOX, ESQ.

Among the rubbish in some crypts or cellars, beneath the old Tower of Durham Castle, several large bones were found; twenty vertebræ, and about the same number of ribs, of enormous size were taken out; and in a crypt or room on the opposite side of the tower, two large jaw bones were laid bare. This latter discovery enabled Mr. Fox to determine, from the form and position of the jaws, that the bones belonged to a spermaceti whale. The discovery excited considerable interest in the town. But while the inquiries, to which the circumstance had given rise, were going on, the Rev. James Raine discovered a curious and interesting letter, in a MS. volume of the late Mr. Surtees' collection, relative to the history of the Castle of Durham, which at once accounted for the discovery of animal remains under such circumstances.

The letter is from John Cosin, Bishop of Durham, addressed to his Secretary, Mr. Miles Stapylton, dated Pall Mall, London, June 20, 1661, and clearly shows that the bones discovered in Durham Castle belonged to an animal cast on shore on the coast of Durham, at Earington, and the date (1661) proves it to be the oldest whale of the kind recorded to have been found on the British coast. The remains of the animal, when collected, were found to consist of twenty-six vertebræ, fourteen ribs, and two lower jaws, of the great blunt-headed Cetodon (*Physeter macrocephalus*).

On the Statistics of British Botany. By Mr. BRAND.

This paper consisted chiefly of remarks on the Catalogues of Plants, on which Mr. H. C. Watson had founded his conclusions in his work on the Geographical Distribution of the Plants of Great Britain. Proof sheets of the Catalogue formed from this source were exhibited to the Section. Also the proof sheet of a catalogue for arranging the Society's General Herbarium.

On the Extinction of the Human Races. By Dr. PRITCHARD.

He expressed his regret that so little attention was given to Ethnography, or the natural history of the human race, while the opportunities for observation are every day passing away; and concluded by an appeal in favour of the Aborigines' Protection Society*.

Mr. J. E. Bowman exhibited specimens of a species of Dodder

* See in this Volume the Synopsis of Grants of Money at Birmingham.

(*Cuscuta epilinum*), first found in Britain, two years ago, by himself; and again in a new locality, within the present month. Having noticed the distinctions between this plant and *C. Europæa*, as well as *C. epilinum* of Weihe, Mr. Bowman described the peculiarities in structure of this singular parasite. When it has fixed itself upon the flax, the root and lower part of the stem shrivel up and die away, and a group of little warts or tubercles is produced from the inner surface of the spire between each head, which strike into the flax and extract its juices. This economy places each head nearly in the situation of an independent plant; so that, if the stem were separated at intervals, each detached portion would continue to flower and to ripen its seed. This view occurred to him, on observing that the stem gradually thickened upwards as it approached each head, and was again reduced to half its diameter immediately above it; each head being thus dependent on its own subordinate system of exhausting suckers. Another beautiful compensation for the loss of the root, and supporting the view just advanced, is found in the succulent nature of the flowers, which are as fleshy as the leaves of the Mesembryanthemum tribe, and contain reservoirs of nutriment to insure the ripening of the seed, and supply the deficiency consequent on the desiccation of the flax.

On the Cultivation of the Cotton of Commerce. By Major-Gen. BRIGGS.

The objects proposed in this paper are—First, to excite inquiry on the various species of cotton plant that produce the cotton of commerce. Secondly, to ascertain the nature of soils adapted to each. Thirdly, to prove the practicability of cultivating the plant in India, for the supply of the British market to any extent. Of the species that produce the various cottons of commerce, we have at present very little accurate knowledge, and this has arisen from the alterations undergone by the plants in the process of cultivation. But there can be no doubt that the plants which produce cotton in America, Asia, and Africa, are of decidedly different species. The plant that produces the Brazil cotton, probably the *Gossypium hirsutum*, grows to the height of from ten to twenty feet, is perennial, and produces cotton with a long and strong staple, and moderately fine and silky. The plant common to the West Indies, said to have been imported from Guiana, is triennial, bearing abundantly a fine silky long staple, and is the *Gossypium barbadense* of botanists. This also is the plant which produces the Sea-island cotton. When this plant was carried from the coast into the interior of Georgia and Carolina, in the United States of America, the seed changed from a black to a green colour, and the staple became shorter, coarser, and more woolly. This plant was afterwards introduced into Egypt, and is the same that produces the Bourbon cotton, cultivated by the French on that island. Mr. Spalding, in a letter alluded to by Mr. G. R. Porter, in his work on tropical productions, records several varieties, attention to which is of the greatest importance to the cultivation, since they vary in the character of their staple, in the shape and

size of their pods, in the hue of the cotton, and in the duration of the plant. The common indigenous plant of India is the *Gossypium herbaceum* of botanists, and differs in appearance from the cottons of the Western world; besides which, there is the *Gossypium religiosum*, producing the brown cotton, extensively grown in China. The former plant is usually cultivated as an annual, but has been successfully treated and grown as a perennial, by the process of pruning down when the cotton is gathered. The produce of this plant is not inferior in fineness, and is superior in point of richness of colour, to the best cottons of America. The staple is however short; and by the great neglect hitherto evinced in picking the produce at the proper time, and carelessness in allowing particles of dried leaves, or the calyx of the flower to adhere to the wool, it fetches a lower price, and is considered an inferior article, in the English market, to the New Orleans and Georgian of America, though really superior in quality and durability. There is another kind of cotton produced from a species in Africa, which Dr. Royle considers allied to the *Gossypium herbaceum* of India.

Several specimens of American soils on which cotton is grown, have been analysed by Mr. E. Solly, and he finds them generally to consist —first, of a preponderating quantity of sand. Secondly, of alumina or clay. Thirdly, of the oxides of iron and manganese, which give the varying colours to the soil. Fourthly, of very small proportions of carbonate and sulphate of lime. And lastly, of organic matter in two states; a fibro-vegetable and a soluble matter forming from four to eight per cent. Soils of this kind, where hardly anything else will grow, are adapted for the cotton plants of America; a fact mentioned by Mr. Porter, and confirmed by Mr. Gray, who was for some years a cultivator of the plant in America. The land on which the indigenous plant of India termed *Gossypium herbaceum* grows, is very different. It is composed chiefly, not of sand, but of the results of the decomposition of trap rocks, the *debris* of the mountains that constitute the extensive trap formation of central India. This soil lies upon or borders on the limestone ; it contains a large quantity of vegetable matter, abounds in oxide of iron, is retentive of moisture, and forms a rich tenacious loam approaching to clay. Such is the soil of the indigenous cotton plant of India, and therefore differs from that of America, so that we ought not to be surprised to learn that all attempts at cultivating the American plant in this soil have failed. But there are in India abundant other soils on which the indigenous plant will not thrive. These prevail in Bengal, on the Coromandel coast, and in fact throughout India. They consist mainly of the detritus resulting from the disintegration of rocks of the primary and secondary formations, such as granite, gneiss, sandstones, with here and there lime, producing a light soil, fertile or otherwise according to the quantity of organic matter it may contain. The indigenous plant will not grow here, but the American plants thrive on it. This has been proved by experimental farms near Bombay, and the Western Coast, in Upper Hindùstan, on the Malayan Peninsula, and on the shores of Coromandel, in all of which tracts of American plants are growing at present in much perfection, though not in quantities sufficient to make any impression on the cotton market of

this country. India could supply all the cotton Great Britain can ever require, even from her indigenous plants, but for local obstacles. The soil, favourable to the growth of this article, however, is situated in a central region removed from the coast, and the trade consequently labours under the difficulty attendant on a lengthened journey by land. This will not be the case when the cotton is grown on the lighter soils of the coast. Here every facility exists for its exportation; for there is no doubt that an article equally good might be obtained at a much cheaper rate than that now procured from America.

On the Introduction of a species of Auchenia into Britain, for the purpose of obtaining Wool. By W. Danson, Esq.

Samples and manufactured specimens of Alpaca wool, in imitation of silk, and (without die) as black as jet, were exhibited; and Mr. Danson stated, that the animals producing it ought to be propagated in England, Ireland, Scotland, and Wales; and to the two latter places the Alpaca is well suited, being an inhabitant of the Cordilleras, or mountainous district in Peru. Importations have already taken place to the extent of one million of pounds, and are likely to increase. There are five species of Llamas, of which the Alpaca has fine wool, six to twelve inches long, as shown by the specimens exhibited, the Llamas, the hair of which is very coarse, and the "Vicuña," which has a very short fine wool, more of the beaver cast. The Earl of Derby has propagated the Alpaca in his private menagerie at Knowsley, and Mr. Danson understood that Mr. Stephenson, at Oban, in Scotland, has a few of these animals. The wool of these animals would not enter into competition with the wool of the sheep, but rather with silk. It is capable of the finest manufacture, and is specially suited to the fine shawl trade of Paisley, Glasgow, &c. The yarns spun from it are already sent to France in large quantities, at from 6*s.* to 12*s.* 6*d.* per pound, the price of the raw Alpaca wool being now 2*s.* and 2*s.* 6*d.* per pound.

On some recent additions to the English Flora. By Charles C. Babington, *M.A., F.L.S., F.G.S., &c.*

The author stated that the following plants had recently been introduced into the list of natives of England, and made some verbal observations upon their claim to be considered indigenous, and upon their specific distinctions: viz.

Nasturtium anceps, *Reich.*
Cardamine sylvatica, *Link.*
Sinapis cheiranthus, *Koch.*
Polygala oxyptera, *Reich.?*
Dianthus plumarius, *Linn.*
Spergula vulgaris, *Boening.*
Stellaria umbrosa, *Reich.*
Hypericum linarifolium, *Vahl.*
Oxalis stricta, *Linn.*
Medicago apiculata, *Willd.*
Arthrolobium ebracteatum, *Desv.*
Myriophyllum alterniflorum, *DeC.*
Callitriche platycarpa, *Kutz.*
Hypochæris balbisii, *Lois.*
Hieracium pelliterianum, *DeC.*
Senecio erraticus, *Bert.*
Orobanche barbata, *Reich.*
Scrophularia Ehrharti, *C. A. Stev.*
Allium sibericum, *Willd.*
Iris tuberosa, *Linn.*

Some Observations on an Apparatus for observing Fish (especially of the family Salmonidæ) in confinement. By Prof. R. JONES.

The points to which attention is required to be directed are the following:—1st. The salmon, the grilse, and the sea-trout, leaving the sea in the autumn, for the purpose of depositing their spawn in rivers, it is desirable to determine whether these are so many distinct varieties, or the same fish in different stages of its growth. 2nd. With regard to the whiting (*Scotticè*, Herling), it is not positively known by fishermen whether it spawns at all, or is merely a young fish, which must undergo a further change before it becomes capable of reproduction. 3rd. The fry, or young fish, in their first descent from the rivers, exhibit certain differences of appearance; but those differences are not such as enable the fishermen to determine the kind or variety (if any) to which the young fish respectively belong. 4th. With regard to the par, or brandling, the questions are, whether it is an adult fish *sui generis*, or the young of some variety, or the ordinary fry, in an early stage of its development. These questions are important, as the decrease of the British fisheries is very great; and, by settling them, such provisions might be made by the legislature as would not only obviate further diminution, but restore the fisheries to their former abundance. A model of an apparatus was exhibited, in which it was proposed to confine the fish, in order that observations might be made upon them in their various stages of growth, provision being made for the admission of sea- and fresh-water, according to the quantity supposed to be required by the fish in their natural state. Mr. Jones then read a letter from Mr. Relph, who had been more than fifty years engaged in the salmon fishery. "In May, 1819," he says, "there were 1700 fry marked at Kings-gate Fishery, near Carlisle; and in the July and August following a quantity of whitings, or herling, were taken, coming from the salt-water, bearing the same marks. These marks were made by cutting away the fin called the dead fin, just above the tail. In September, 1821, a grilse was caught bearing the mark, and weighing 7 pounds, 6 ounces; so that from the time it was marked its average growth had been one ounce per week. There were also several salmon taken bearing the mark, and weighing from 10 to 16 pounds."

Observations on Beroë pileus. By ROBERT GARNER, ESQ., *F.L.S.*

The author has not been able to observe true luminosity in this animal, even in a perfectly dark room; but in an obscure room it exhibits peculiar changeable colours.

The vibrations of the external cilia continue after these parts are separated from the body, with almost undiminished rapidity for several hours. The circulation of aqueous fluid in the internal canals of the animal is attributed by the author to the action of minute internal cilia, situate on the parietes of the cavities. They may also well be seen on the external surface of the stomach where it is washed by the fluid of the central canal. There is sometimes an appearance of one or more

small bodies varying a little in size and shape attached to the external parietes of the stomach, not within it, but apparently in the tissue of the animal.

Mr. Lankester made a communication on some specimens of the White Bream. Amongst the fish taken at Campsall, is one resembling the White Bream (*Abramis blicca*). These fish vary very much, and do not quite agree with the descriptions given by Mr. Yarrell; from which certain distinctions were pointed out by Mr. Lankester.

MEDICAL.

Abstracts of a remarkable case of Rupture of the Duodenum, and of some other interesting Cases. By Sir DAVID J. H. DICKSON, *F.R.S. Ed., F.L.S.*

1st. Richard Hawkins,—M. æt. 40, was admitted into this hospital 3rd March, 1839, at three o'clock P.M., and died before midnight. The symptoms were, severe pain in the region of the cæcum and ascending colon; quick, restless, impatient manner; pale, haggard, anxious countenance; short, hurried respiration, and very weak, quick, irregular pulse. Depletion and aperients had been resorted to before, and leeching. Fomentations and purgatives, enemata, &c., after admission, without affording any relief, and at half-past eleven he expired. It was ascertained that he had been drinking and wrestling, three days previously, when he was thrown with violence, backwards, on the breech of a gun; but he did not suffer much pain until the morning of admission, when he felt excruciating pain whilst straining at stool. Dissection, 40 hours *post mortem*, discovered the following lesions. The stomach and bowels were distended with flatus, and there was some gas in the cavity of the abdomen. The transverse and descending colon were much contracted; a quantity of ingesta had escaped from four perforations near the termination of the duodenum, three of which were large enough to admit the end of the finger, and from one to two inches apart. The mucus and muscular coats of this gut were pellucid and attenuated, as having undergone ramollissement and absorption, in consequence of which, the peritoneal coat seemed to have given way from distension or mechanical violence. From its peculiar course, and the manner in which the duodenum is bound down, Sir David Dickson deems it fair to infer that this gut may be more liable to injury, from particular causes, than the more free and floating intestines,—such as require violent exertions and contortions. It is known that sudden death frequently follows certain feats of tumbling, horsemanship, &c., accomplished by retroversion of the body; and, if exami-

nation were oftener made, he thinks it probable that similar lesions would be found, or might exist without being detected, from the examination not being sufficiently minute; and hence this cause of death may more frequently occur than is generally supposed.

The next case detailed was one of *Ileus*, with enormous distension of the cæcum, which occupied the situation of the transverse colon. The usual symptoms of *Ileus* were present—viz. obstinate constipation, which had lasted for five days, stercoraceous vomiting, and singultus, &c. The ilio-cæcal valve was found to be much thickened and diseased, and nearly as hard as cartilage; notwithstanding, a strong membranous band, the product of former inflammation, attached to the lateral wall and to the mesocolon, extended across the *ilium*, so as to produce strangulation; yet the lower portion of this gut, with the cæcum (which had a black sphacelated appearance, and was much distended,) had been forced upwards into the above position.

The next, was a case of *Intermittent Coma*, from diseased brain; and remarkable for the alternations of coma, and excitement. The *post mortem* examination showed the arachnoid membrane to be opaque, and raised from the brain by a gelatinous deposit. A considerable effusion of blood was found at the base of the brain, produced by the rupture of a true aneurism of the anterior artery of the cerebellum, near its origin from the basilar artery. The coats of the artery exhibited distinct ossific deposits; the cerebellum on the left side was wasted, soft and pulpy, and looked like curdy pus. The aorta was extensively invaded by osseous degeneration; bony scales as large as sixpences being separable, and its elasticity was consequently much impaired.

Another case of *Arachnites*, which, besides the usual cerebral appearances, exhibited an extensive deposition of little semicartilaginous bodies, in the subserous tissue of the abdominal viscera;—and other instances of *Coma* were adduced in which depositions of a cartilaginous or tubercular nature were found in different parts of the brain, abdomen, &c.

In a case of *Phthisis*, the *foramen ovale* was found open, without *Cyanosis* having been produced; and the patient, a pensioner, had completed his due period of servitude, and had risen to the rank of sergeant, without (until latterly) having suffered any particular inconvenience from the communication between the two sides of the heart.

The next case detailed was one of *Phlegmonous Erysipelas*, occupying the arm and thoracic muscles of the left side, and remarkable for its extent and extreme rapidity;—for in three days, besides intense inflammation (followed by effusion) of the pulmonary, costal and diaphragmatic pleuræ, the vessels, nerves, and muscles of the neck, thorax and shoulder, down to the elbow joint, were invaded by purulent infiltration.

Two cases of severe abdominal disease were also detailed. The one was a case of constant vomiting from *Chronic Pancreatitis*, which had degenerated into scirrhus of that organ, and of the pylorus, together with deep, and extensive ulceration of the duodenum.

The last case was a most extraordinary instance of *Peritonitis*, and

Scirrhoma, from the effects of which the patient was reduced to such extreme emaciation as to resemble, or rather to surpass, that of "*L'Anatomie Vivante*." Besides effusion in both cavities, and the usual effects of intense peritoneal inflammation, the stomach, liver, pancreas, colon, &c., being accreted into one mass of disease,—in the subserous cellular tissue of the first, and of the large intestines especially, there was extensive ichthyoid deposit of semi-cartilaginous matter, by which the calibre of the descending colon especially, was so much reduced, that, when cut across, it appeared as if encircled by a broad ring (in some places upwards of an inch in thickness), of a dull white, yet glistening, fish-like substance, but fibriform, and by the interposition of which the serous was so completely disconnected from the subjacent coats, that large portions of the latter could be drawn out from the former, with the utmost facility. Want of space precludes a more copious detail of the morbid anatomy, or any further observations on the extent of this heterologous formation, and which was very abundant in the great end of the stomach, intestines, and other viscera. Taken altogether, the case is probably *unique*, and has no prototype on record.

On the Treatment of Capsular Cataract. By R. MIDDLEMORE, ESQ.

The object of this communication was to introduce to the Section an instrument to facilitate the operation of extraction, without interfering with the transparent structures of the eye. The instrument consisted of a needle, accompanied by a small forceps, the former capable of being withdrawn, leaving the latter to be fixed on the opaque membrane, and then withdrawn through the sclerotic, through which the needle had been introduced. The author presented some general views of the disease in question, and compared the methods of operation commonly used with that to be followed with his own instrument.

On an Operation for Artificial Pupil. By R. MIDDLEMORE, ESQ.

About three years ago much injury was done to the face of J. S., from an explosion of gunpowder. After recovery of the other parts, the eyes were found to be in the following condition:—the right was completely collapsed, the left was staphylomatous, the lens adhering to the staphyloma, but transparent; the lower half of the cornea was opaque, the upper half transparent, but vision destroyed, from the closed iris being opposite to the transparent portion of the cornea. The first effort was to remove the staphyloma, which was done by repeated puncturing of it with a fine needle. When the process of removal was so far completed as to permit the operation for artificial pupil, the iris was drawn through a small section of the cornea: it bled freely; but on the subsidence of the hæmorrhage and irritation, a sufficient and well-defined opening was found in the iris opposite the transparent portion of the cornea. The external portion of the iris was allowed to remain strangulated by the incision. The patient has already in a

great degree recovered his sight, so as even to distinguish large print. He is still under treatment.

Results of researches on the Anatomy of the Brain. By Dr. Foville *(of Paris).*

He commenced by urging the advantages of examining the structure of the brain by manual separation rather than by section, and gave credit to our countryman Willis, as being the first advocate of this method. He showed that the spinal marrow consists of two lateral portions, united by two commissures, between which, on the median line, there exists a double layer of white matter, analogous to the ventricle of the septum lucidum. He pointed out a remarkable difference of structure in the lateral parts of the spinal marrow, between the roots of the nerves, which is rendered most evident by maceration in water, after previous maceration in spirit. He next described the medulla oblongata. Tracing the crura cerebri to the brain, he showed them to consist of two parts,—the one going to the thalamus opticus, the other to the corpus striatum, where they constitute the white matter; passing through the middle of those bodies, at the upper and outer limits of which they divide into three layers,—the superior, passing upwards and inwards, meets its fellows on the median line, and forms the corpus callosum; the second, or middle, is expanded in the hemispheres, which it constitutes, by lining the cineritious matter of the convolutions; the third, or inferior, and by far the smallest layer, passes to the outer side of the thalamus and corpus striatum, meets its fellow inferiorly, and, ascending with it, forms the septum lucidum. In addition to these facts, he stated his more recent discovery, of several nearly circular systems of white fibres connecting the expansions of the superior part of the crus cerebri, which, from their connexion with the olfactory and optic nerves, and also with the posterior part of the spinal marrow, appear to be essentially devoted to sensation. He also stated his fully-confirmed observations, that the pathological affections of the thalamus influence the movements of the opposite side of the body, as those of the corpus striatum do those of the lower extremity. He noticed a similar connexion between the lesions of the cornu ammonis and the motions of the tongue. He combated the idea, that the frontal, parietal, and occipital protuberances, are dependent on special development of the corresponding parts of the brain, but are rather to be attributed to the distension of corresponding parts of the ventricles. After the reading of the paper, Dr. Foville demonstrated the leading facts alluded to, on the recent brain.

On the means employed to suppress Hæmorrhage from Arteries. By Dr. Macartney.

The progressive improvements made on this subject constitute some of the most interesting and instructive pages in the history of surgery,

inasmuch as they were delayed for ages by the existence of the theory, which considers inflammation a sanative process, and as their successful applications furnish so many proofs of the falsity of this opinion.

Dr. Macartney, after exposing the errors in practice to which this error of hypothesis has led, explained some new views on the subject. In his opinion the common ligature, when it does *not* succeed, in almost every instance fails from creating irritation, and consequently a dilated state of the arteries, or their ulceration; although much depends on the plan of after treatment in repelling inflammation, still the parts feel the presence of the thread, however small, as an extraneous body, and therefore do not perfectly return to their natural state of feeling and action, until the ligature be removed.

"It is well known that metallic substances lie in the living structure, without exciting in it any irritation, or efforts to expel them; I therefore conceived that a ligature made of leaden wire might be employed with many advantages. I accordingly made the experiment of tying both the carotid arteries of a dog with a filament of lead. I cut off the ends of the leaden wire close to the artery, and healed the wounds over them. I killed the animal some weeks afterwards, and found both the vessels obliterated. One of the ligatures remained on the artery, and the other had been removed by *interstitial* absorption, and lay on the side of the vessel. Both were inclosed in a capsule of transparent cellular membrane; no lymph had been shed, except what was sufficient for the consolidation of the divided coats of the arteries. As the presence of the lead had not created irritation, lymph was not required to limit or arrest inflammation."

(The appearances were represented in accompanying drawings.)

"I afterwards tied both the jugular veins of the rabbit in the same manner. The animal died in two days of apoplexy, as was expected; but no appearance of inflammation existed around the veins, and both ligatures remained on the vessels. These experiments were made before Dr. Dieffenbach employed the metallic ligature for closing the fissures of the palate. A considerable improvement has since been made by Mr. Weiss, by substituting soft metal for the leaden wire. Weiss's ligature is so flexible, that it admits of being tied in a knot. I have since had a silver needle, and also a steel one made, for receiving the end of the metallic ligature, and passing it under an artery."

Several cases are recorded of even large arteries being broken, in lacerated wounds, without yielding any hæmorrhage. In experiments to discover the reason of this, it was found that, by pulling the artery slowly until it gave way, it yielded a few drops of blood, after which no more issued; and on examining the artery immediately after the experiment, the matter was fully explained; it was found that the middle and internal coats had first been broken. They presented an inverted edge or burr within the tube; and the cellular coat, as it admits of more extension, had been drawn out into the form of an elongated cone before it gave way; therefore the hole left at the bottom of the cone was very minute. There seemed to be no disposition in the cone of cellular substance to fall back into the cylindric form; but

if there had, it would have been resisted by a coagulum of blood filling it, and which had formed at the instant of the experiment; and even a little on the end of the cone for the purpose of soldering up the minute aperture left by the rupture of the cellular sheath. We perceive, therefore, that the hæmorrhage from even a large artery may be arrested, by the strength of the cellular tunic, when aided by the disposition of the blood to rapidly coagulate. This is soon followed by the tendency which all arteries possess to contract, when there are no parts beyond them that require a supply of blood. We thus find the explanation of the vessels on the face of a stump having a less tendency to bleed, than when the same arteries are tied in aneurism.

Dr. Macartney read a letter from Mr. Darby, of Bray, near Dublin, describing the entire success which attended the application of a mode of treatment identical in principle with the views above explained, in a case of amputation of a child's hand. No ligature or pressure was used. The stump was covered with a light piece of lint frequently dipped in cold water, and on the tenth morning the wound was almost perfectly healed. On these grounds the author expresses hopes that the day will arrive in which the use of the ligature will become unnecessary. He is fully persuaded, that in the operation for aneurism, (provided the collateral vessels were enlarged,) by making a simple incision and uncovering the artery, and treating it afterwards by cold, rest, and elevated position, and thus producing union without inflammation, as in other cases where wounds heal by the process of approximation and natural growth, the main artery of the limb would become, from the sense of exposure, by degrees impervious, which would be evidently preferable to the sudden interruption of the circulation.

Another case is mentioned by Dr. Macartney, occurring in his own experience, where the application of ice to a wound of the femoral artery stopped hæmorrhage, when other means had been unsuccessfully resorted to.

On the Sounds produced in Respiration, and on the Voice. By Peyton Blakiston, *M.D.*

Dr. Blakiston commenced by showing that the respiratory sound, coarse and intense, when heard in the trachea, became gradually weaker and softer as it approached the periphery of the chest, at which point the sound, during expiration, had almost totally disappeared. The air, in passing along the trachea and bronchial tubes, would meet with solid obstacles, and therefore be thrown into sonorous vibration at every alteration of direction. The divergence of sound, caused by the subdivision of those tubes, and the diminution of their calibre, would necessarily tend to soften and weaken the respiratory sounds from the trachea towards the air vesicles; but the sound produced by inspiration was carried up to the ear placed on the chest by the current of air during that act, while that produced by expi-

ration was carried quite in a contrary direction: hence the difference in intensity. It was next shown that bronchial respiration, occasioned by solidification of a portion of lung, did not take place in the tubes leading solely to that portion, as had been supposed by Andral and Laennec, because no current of air could take place in tubes whose vesicular extremities had lost their expansibility by which the current was produced; but that it took place in tubes leading to healthy expansible vesicles; and the ear being brought into contact with the sides of these tubes, perceived the coarse and comparatively undiverged sound of the air passing and repassing in them. It was contended that no sensible part of the sound of vesicular respiration was produced in or around the vesicles, or by the rubbing of the pleuræ, otherwise it would be clearly heard in expiration; nor in the mouth or fauces, otherwise stertorous breathing would increase its intensity; that consequently it chiefly originated in the bronchial tubes, a supposition rendered very probable by the fact, that it is much affected by sonorous and sibilous *râles*.

The voice being an instrument of the membranous reed kind, Dr. Blakiston then detailed a series of experiments he had made with different kinds of pipes on the wind-chest of an organ, which led him to conclude that the quality of tone of wind instruments became uniformly more coarse and buzzing in proportion to the strength of the blast, and the thinness and elasticity of their sides; in other words, in proportion as the instrument itself entered into strong vibration. Some curious illustrations of the manner in which interference and jarring was produced between these solid vibrations of the instrument, and those of the air contained in it, were then given, and it was stated that this law was applicable to every wind instrument. Among other instances adduced was that of the flute, in which the upper notes are clear; and the lower ones, produced by powerful sonorous waves, affecting the material of the instrument, are coarse and buzzing. It was shown that both kinds of vibration were concerned in its formation of the voice, and that hence, when heard over the larynx, it was perceived to be coarse and intense. In proportion, however, as these vibrations travelled downwards toward the air vesicles, they were deadened, the aërial waves by the opposing current of expiration, and the solid ones by the increasing mass of the spongy non-homogeneous lung: hence, at the periphery of the lungs, no resonance of the voice could be detected.

When however a portion of the lung became solidified, the current of expiration leading from it was stopped, and the spongy lung was transformed into a more homogeneous, and therefore better conducting substance: hence the voice resounded strongly, and its quality sometimes became so coarse as to produce a stinging sensation in the ear. Dr. Blakiston stated that he was now employed in investigating the subject of the propagation of sound through different media.

Notice of an extraordinary case of Spina bifida. By Mr. Evans.

The patient was a boy of twelve years of age, enjoying excellent general health in other respects; he was strong and active, but his head seemed enlarged from chronic hydrocephalus. The tumour occupied the lumbar regions, was semi-transparent, and the size of a child's head.

Observations on Poisoning by the Vapours of burning Charcoal. By Golding Bird, *M.D.*

Dr. Bird stated, that he was induced to state the result of some experimental investigations on this subject, from the discordant opinions hitherto published on the various questions connected with it in a toxicological point of view. An opinion has been held, that vapours of carbonic acid were more injurious when produced by the combustion of coal and charcoal, than from any other source, on account of the admixture of light carburetted hydrogen gas. This opinion he dissented from, as it was well known that in coal-mines the fire-damp, as this gas was called, was inhaled with perfect impunity. To ascertain the *modus agendi* of the gas when inhaled, he made numerous experiments, by immersing animals in different mixtures of it and atmospheric air, as well as in the pure gas. In the latter case, the animals died asphyxiated, as when immersed in water or mercury, the spasm of the glottis preventing any portion of it from being inhaled. If not more than twenty-five per cent. be present, then respiration will go on, and its true poisonous effects takes place. As to the amount of this gas necessary to produce fatal effects, Dr. Bird found that, as a general rule, any quantity above $3\frac{1}{2}$ per cent. was capable of producing death. Two opinions prevailed on the nature of these properties: the first was, that the gas acted negatively, as pure nitrogen or hydrogen is known to do, by preventing the due supply of oxygen. To test this opinion, he formed a mixture containing twenty-one parts of oxygen, and seventy-nine of carbonic acid, and death followed instantly from immersion in it; and the same result followed when the proportions were reversed, although a taper burned brilliantly in the latter combination; showing, that the burning of a light in any suspected situation is not always a safe test of the absence of danger. The second opinion is, that this gas, when respired, exerts a specific poisonous action on the nervous system. This latter, Dr. Bird adopts, from various considerations drawn from his direct experiments, and from the symptoms observed in numerous cases. These are principally those denominated cerebral, such as head-ache, vertigo, suffused eyes, mental horror to an intense degree. Even with these symptoms, respiration may go on freely. Death is frequently preceded by vomiting, which is a marked symptom of cerebral disease. In cases where recovery has taken place, the sequelæ are decidedly of nervous character: they have been, partial paralysis, dumbness, and idiocy; and this poisonous effect he thought took place independently of absorption, from its immediate effects on the nervous system, to which it was applied. Death has also been induced by its external application

to the body, without its being, at the same time, respired. Dr. Bird related some experiments of Dr. A. T. Thomson, in which the pain of inflamed surfaces was instantly removed on their being plunged into carbonic acid. He dwelt on the pathological effects of the gas as exhibited after death, and concluded by pressing the importance of minute *post mortem* examinations in every case of death from this cause coming under the notice of medical men*.

On the Rules for finding with exactness the Position of the Principal Arteries and Nerves from their Relation to the External Form of the Body. By Dr. MACARTNEY.

Painters and sculptors have laid down, for the improvement of their arts, the proportions which belong to the external figure of the human body, and in doing so have demonstrated a very interesting fact, namely, that these proportions are in general regulated according to the primary relations of duplicates or thirds, and the multiples of these. Dr. Macartney has discovered that a similar law of proportion prevails with respect to the internal parts of the body, more particularly with regard to the course of the trunks of arteries and nerves in relation to the limits of the external form. Sometimes these parts take a middle line along the limb for some distance, as may be observed in the trunk of the sciatic nerve, but more frequently they occupy lines dividing the external form into thirds, or proceed from the median line of one side of an extremity to the middle of the opposite side, or they may pass from the middle to the division into thirds, or from a point placed on a line dividing the external form into three equal parts, and then approaching the middle so as to form with the fellow two parts of a triangle. Let us take for instance the course of the arteries in the superior extremity. The subclavian artery first passes obliquely behind the middle of the anterior curvature of the clavicle, to the middle of the axilla. The brachial artery proceeds from the middle of the axilla, to gain a line dividing the inner third from the two outer thirds of the upper arm, and ending in the middle position in the bend of the arm. The radial artery is properly the continuation of the trunk, and passes under a line drawn from the centre of the fold of the arm to arrive at the place where we feel the pulse, which is on a division of the external form of the wrist into fourth parts, or the duplicates of two. The ulnar artery pursues almost exactly a similar course on the opposite side of the fore arm, and the inter-osseous takes a middle line. The superficial palmar arch corresponds in its greatest extent to a line dividing the palm into two equal parts, and the deep-seated arch exists under a line which would divide the upper third of the palm from the two lower thirds.

The occipital arteries, after they emerge from the muscles, furnish us with an example of vessels proceeding from the division of the external form into thirds, towards the median line of the head.

* Dr. Bird's Essay has been published at length in the Guy's Hospital Reports for October 1839.

The course of the trunks of arteries (with two or three exceptions,) is as much in straight lines from one part to another as that of the nerves, when the vessels are not displaced by dissection and forcible injection.

The position of the three facial nerves, where they emerge from their foramina, is almost with perfect exactness upon vertical lines, which would divide a well-proportioned face into thirds; but for the purpose of fixing the points at which these nerves may be divided, the author has laid down the following rules: a vertical line, drawn so as to pass midway between the external angle of the orbit and the middle line of the forehead, will cover the supra-orbital nerve as it escapes from the notch or foramen, as the case may be, on the superciliary ridge. For the infra-orbital nerve, the following lines may be drawn: 1st, one perpendicular along the outer side of the second bicuspid tooth; this will divide the orbit in the middle. 2nd. A line from the external angular process of the orbit to between the two middle incisors of the upper jaw; this line covers the course of the nerve as it comes out of its foramen. 3rd. A line may be taken from the lower part of the internal angular process of the *os frontis* to the angle of the lower jaw; this line passes across the nerve a few lines beyond its exit from the infra-orbital hole, and indicates the direction in which the nerve should be cut. 4th. If two lines be drawn, one from the internal, the other from the external angular process, so as to meet and form an equilateral triangle with the horizontal line from the same points, the inferior angle will determine the distance of the foramen from the inferior margin of the orbit. By the intersection of these lines and their direction, the most perfect knowledge may be obtained of the position of the infra-orbital nerve for the purpose of its division.

The mental nerve, immediately on the outer side of its foramen, is crossed by a line dropped vertically from the superciliary notch. The height of this nerve on the jaw will vary according as age may have changed the form of the bone; but this is of no importance, as the division of the nerve is best effected on the inside of the mouth, which produces no deformity.

Dr. Macartney further observes, that the same primary relations regulate all the progressive, and many other movements of all animals provided with extremities. They also constitute the foundation of the measure in music, and the rhythm of language. All musical time consists essentially of divisions or bars, containing two or three notes, or multiples of these numbers, and in no other proportions are we able to count it. Our powers of perception even are subjected to the same law of proportion. If we attempt to look at more than two or three objects at the same moment, and without shifting the attention from one to the other, we find that it is impossible to distinguish or compare their differences except by making parcels of them, and then each of these aggregations represents an unit. The author adds, that the combinations of doubles and thirds produce the proportions in all architectural forms, which yield us the most pleasure to contemplate.

Dr. Macartney began his observations on this subject as early as the year 1798, and has now had forty-one years' experience of the

advantages to be derived from the possession of positive guides to the situation of the nerves and arteries which may be concerned in accidents and operations. Mr. Alexander Walker also studied the subject as early as 1804.

Dr. Macartney then described a remarkable case showing the truth of his views, and adduced examples from his own experience of the facility of applying them in practice.

On the Cause of the Increase of Small-Pox, and of the Origin of Variola-Vaccinia. By Dr. INGLIS.

Dr. Inglis stated, that variola was every year upon the increase, the cause of which was, not that vaccination was inefficient, or that the virus had degenerated, but that, from a long immunity from small-pox, the public had ceased to think vaccination necessary. He adduced proofs from the Cow-pox Institution of Dublin, from foreign reports, and from the innumerable cases of successful re-vaccination, that the vaccine virus had not degenerated, but that the human system did undergo a change during some unknown number of years. In Ripon, during the year 1837, variola prevailed extensively as an epidemic, and Dr. Inglis observed at that time innumerable cases of varicella; those affected with chicken-pox were principally children upon whom vaccination had not recently been performed, and those who had chicken-pox, without vaccination, seldom contracted small-pox. The two diseases appeared to Dr. Inglis to arise from one cause. Many cases, to prove the convertibility of the one disease into the other, were adduced. Dr. Inglis, having full faith in the efficacy of vaccination and of re-vaccination, after first inserting the vaccine lymph, inserted into his arm in several places, the virus from variolous patients in different stages of the disease, and, in one instance, from a patient who was dying from the disease, but in none of them did he succeed in inducing an eruption; the inflammation and pruritus was considerable for a day or two, but then gradually subsided. That the vaccine virus, therefore, decreases in its preventive influence, is a supposition at least difficult of proof; for, from the beginning, this prophylactic power was imperfect in different degrees, and even an attack of small-pox itself is no certain security against a second or even a third attack. The next point in the paper was to show that the two visitations of small-pox and vaccination could and did go on in the system at one and the same time, distinct cases of which were brought forward. Now, since two dissimilar contagious irritations cannot run their course together without the one impeding the other for a time, Dr. Inglis was led to suppose that variola and variola-vaccinia had the same common origin, or rather that vaccinia sprung from variola. The paper concluded by the following brief summary:—1st. That small-pox is decidedly on the increase, and that during each successive epidemic there is an increase of variolous patients from amongst those who were vaccinated in infancy. 2nd. That the vaccine virus is as effectual now as ever it was, but that re-vaccination is necessary after a period of years, as yet unknown. 3rd. That the same

cause which produces small-pox during a variolous epidemic in the unvaccinated, may and does give rise to chicken-pox in the vaccinated. And 4th. That there is every reason to believe that cow-pox had its origin in variola.

On the New Vaccine Virus of 1838. *By* J. B. Estlin, *F.L.S.*

The author, in common with many of his professional brethren, having long been dissatisfied with the vaccine lymph furnished by the National Vaccine Establishment, and believing that small-pox after vaccination had become an event of more frequent occurrence than was the case formerly, availed himself of an opportunity of procuring some fresh virus from a dairy farm near Berkeley, in August 1838. Particulars respecting the source of the virus are recorded in the London Medical Gazette for September 1838. The offer of a supply from this stock of lymph having been made through the medium of the Medical Gazette to medical gentlemen connected with public vaccine institutions, numerous applications were made to the author for it, and in the course of a few months it was in extensive use throughout England; its employment at the principal vaccinating establishments in the populous towns of Birmingham, Bristol, Liverpool, and Manchester, had it been used nowhere else, would give some importance to the history of its origin and progress.

The author had carefully preserved this matter distinct from every other, and had watched its course from the cow for twelve months, through nearly fifty successive inoculations.

"During the first three months of the employment of the new lymph, in a large proportion of cases, a degree of intensity of action was observed quite unusual in the lymph previously used. After the areola had formed, which seldom commenced before the ninth day, the inflammation was considerable; the constitutional symptoms were often severe; and after the subsidence of the areola, in consequence, apparently, of the depth in the cellular membrane to which the vesicle had extended, the separation of the crust was attended by a secondary inflammation very much resembling the process which takes place in the throwing off of an eschar made by caustic potass. In many examples, after the coming away of the crusts, deep circular ulcerations remained. Erythematous attacks affecting the body were not unfrequent, and occasionally erysipelas came on: vesicular eruptions also occurred: many cases of abscess in the axilla were met with, and sometimes a succession of boils and small abscesses appeared on the arm and body." Several medical practitioners, from witnessing the severe effects of the virus, were induced to discontinue it; and others who had employed it, had to endure a portion of obloquy from their less intelligent patients on the supposition of their having been inoculating with improper virus. The irritability of the vesicle at this period was so great that children beyond a few months old usually rubbed and broke it, so that excepting in young infants, a perfect vesicle was not often met with on the eighth day, and it was found expedient to inoculate in not more than two places.

About the month of February in the present year, the vesicle had acquired a greater degree of firmness, and was seldom found to be broken during the first week. The author observes, in reference to its character at the time the paper was read, "at the present time a more decided change has occurred; the vesicles usually remain perfect through their whole progress: erythematous eruptions, as well as abscess in the axilla, and severe constitutional disturbance, are comparatively rare. I am then able to state as a decided fact, that the lymph, now forty-eight removes from the cow, has lost much of the intensity it possessed when only fifteen degrees from its original source; while at the same time it retains all those appearances which Jenner describes as characteristic of a perfect specimen of the disease."

Full confirmation of the important fact was afforded the author in the course of his vaccinations, that from some peculiarity of constitution, lymph taken from a perfect vesicle will produce a pock deficient in some of the characteristics of the genuine vaccine disease, and that matter taken from this imperfect vesicle will produce others that are defective, so that in two or three transmissions the virus may become totally degenerated. Such a fact being satisfactorily determined, it is idle to theorize upon the effect which frequent transmissions of virus may have in "*humanizing*" it: the practical point suggested by it is of most consequence—the importance of selecting perfect vesicles only for future vaccinations.

The paper concluded with a notice of the valuable series of experiments made by Mr. Ceely of Aylesbury, and brought forward in a Report of the Vaccination Section of the Provincial Medical Association at their late meeting at Liverpool, by which it was proved that small-pox matter inserted into the cow produced a vesicle in no respect distinguishable from ordinary cox-pox, and yielding lymph, which, when transferred to the arms of children, had produced the genuine vaccine vesicle through twenty-four transmissions: thus confirming Jenner's opinion of the identity of small-pox and cow-pox, and making a most important contribution to the practice of vaccination by discovering an easy method of producing the vaccine disease at any time when small-pox prevailed.

Sir James Murray continued the subject of his paper in the *Seventh Report* of the Association, proving, by dissection, that cases of torturing neuralgia depended in some instances upon the irritation occasioned by a frost-work of microcosmic crystals deposited in the nervous membranes; which crystals, like the crystals of tinea and lipra, or the discharges from ill-conditioned ulcers, he found to contain a large proportion of urinary salts. Proofs were adduced that other complaints and nervous affections originate from acids forming in the stomach, and impregnating the tissues and circulating fluids with acrimonious deposits, such as urate of soda: hence he condemned the internal use of soda in such cases.

It was further shown, that in those gouty and urinary sediments which prevail after deranged digestion, *fluid magnesia* was found for

thirty years infinitely the most effectual remedy, as also for those acids which soften the bones and occasion rickets and debility. This fluid is exempt from the danger of forming concretions in the interior cavities, as was so often the case when crude magnesia was taken.

The fluid magnesia was then tested, and gave the greatest satisfaction to the Section. Sir James appealed to many members then present, from Dublin, Belfast, Edinburgh, and Glasgow, to testify that he had spent thirty years in bringing this elegant preparation into perfect condensation, and into a state exempt from all impurities.

On Alkaline Indigestion. By R. D. Thomson, *M.D.*

The author stated that he had brought this subject before the British Association at Bristol, but that since that period he had not only from ample experience confirmed the results of his former inquiries, but had elicited several other conclusions of importance. In the healthy state, there is no doubt that during a portion at least of the process of digestion, the contents of the stomach are in an acid state. Whatever this acid may be, there is no doubt that when it accumulates to a certain extent, the stomach can no longer sustain it, and disease ensues in the form of heartburn, acid eructations, &c. Where the contents of the stomach assume any condition offensive to that organ, either from too much acid or from too small a proportion, the stomach, in many cases, ejects a clear fluid, which Dr. Thomson has found to be accompanied by different symptoms, according to the chemical re-action of the fluid: thus in heartburn an acid fluid is ejected, but without any cessation of pain in the stomach; while, on the contrary, if a neutral fluid be ejected, according to the experience of the author, the pain is alleviated on the instant that the fluid is got rid of. This is a more rare case of indigestion, but the author has met with it several times. It may be termed *Neutral Indigestion.* The third form of indigestion which Dr. Thomson has met with is the alkaline state of the fluid ejected. He terms it *Alkaline Indigestion.* The peculiar features of this disease are a violent pain in the region of the stomach, accompanied sometimes with a feeling of fainting, headache, and more rarely an inclination to vomit. Suddenly a sensation of spasm comes on, as if some contraction were taking place, and the patient speedily finds his mouth full of water, which he is obliged to empty. This operation he has no sooner performed, than he requires to repeat it, and at last a continuous stream flows from his mouth, which endures for some time, when it ceases, and along with it the pain of the stomach. This, together with the chemical re-action of the fluid ejected, appears to distinguish, in a very complete manner, alkaline and neutral indigestion from the acid state. The distinction is the more important, because these different forms require, in some measure, opposite modes of treatment. With regard to the cause of the alkaline re-action, Dr. Thomson stated, that after evaporating the

fluid emitted from the stomach, and igniting the residue, he had obtained, by crystallization, fine crystals of carbonate of soda. The presence of these, however, he ascribed either to the decomposition of common salt by the process, or to the previous existence of lactate of soda in the fluid. He was more inclined to attribute it to the former source, because the quantity of crystals was so very considerable. Dr. Thomson stated that the ejection of these fluids from the stomach was much more common than was usually imagined, as out of forty or fifty patients admitted daily at the Blenheim-street Dispensary, in London, he frequenlty found one or two affected with such symptoms.

On the Red Appearance on the Internal Coat of Arteries. *By* Joseph Hodgson, *F.R.S.*

This appearance, he stated, did not depend on inflammation in every instance, and should be carefully distinguished from it; it might occur extensively, or in small patches, or in different parts of the same subject, presenting different shades of colour. It was found in subjects of all ages, in healthy as well as morbid coats, in the lining membrane of the heart, and of the veins, but less frequently in the latter. It may be found when blood is present in those cavities after death, or where they are completely empty. Mr. Hodgson related the experiments of Laennec and Andral, which proved that this red appearance might be communicated after death by immersing the vessels in blood. As to the efficient cause, he stated that it might proceed from imbibition, in the same manner as we find the neighbouring membranes stained with bile from the gall-bladder and its ducts; the first changes towards decomposition and putrefaction might allow of it more readily. Some writers look on it in every instance as the result of inflammation; slight modifications of vitality may permit its occurrence during life, as we find it, where chronic inflammation has existed, giving rise to deposits of an atheromatous matter. When dependent on inflammation, it will be found affecting the inner coat only; but when on other causes, it will often pervade the elastic or middle coat as well as the serous. Finally, he stated that it might be found depending on the co-existence of those causes which were capable of producing it singly.

On the Respiration of Deteriorated Atmospheres. *By* C. T. Coathupe.

The experiments were instituted to determine whether the injurious effects which have followed the respiration of charcoal vapours had depended on carbonic acid, as was generally thought, or on the specific agency of some other volatile product. The volatile products of the combination of charcoal he stated to be as follows:—Carbonate of ammonia, hydrochlorate of ammonia, sulphate of ammonia, volatile

empyreumatic oil, carbonic acid gas, carbonic oxide, oxygen, nitrogen, aqueous vapour.

From a number of experiments on the elimination of carbonic acid during respiration, he arrived at the following results:—that 266·66 cubic feet of atmospheric air pass through the lungs of an adult in twenty-four hours, of which 10·666 are converted into carbonic acid, yielding 5·45 ounces of carbon, or 124·628 pounds annually. The average amount of carbonic acid found in atmospheric air in which animals had expired was found to be, for warm-blooded animals, 12·75′ per cent., for the cold-blooded animals, 13·116 per cent. When the animals were removed, on becoming comatose, the average amount of carbonic acid was found to be 10·42 per cent. On confining a taper until its extinction, the quantity of carbonic acid found was 3·046 per cent. From hence it would appear, that an atmosphere which has ceased to support combustion can support animal life for some time.

Report of Ten Cases of Calculus treated by Lithotrity. By Dr. COSTELLO.

The patients were of ages between fifty-three and seventy-six, the stones varying in size from that of a pigeon's egg to that of a hen's egg. The lithotrite was successively applied at sittings of from thirty to fifty seconds. Dr. Costello strongly insisted on the necessity of this point, especially at the commencement of the treatment, as the constitution is thus saved from the shock and re-action which follow protracted operation. In one of the cases the collected fragments of the removed calculus filled a bottle capable of containing at least four fluid ounces. The patient had suffered upwards of ten years; during the treatment he superintended the farming of his estate as usual, without any inconvenience; the entire of the ten cases were cured.

On the Cellular Structure of the Ivory, Enamel, and Pulp of the Teeth, as well as of the Epithelium, and on some other interesting points of Odontology. By A. NASMYTH, *M.R.C.S., F.L.S., F.G.S., &c.*

On Instruments made from Softened Ivory. By Dr. LUDWIG GÜTERBOCK.

Instruments made of softened ivory were presented to the Section, and described by the author. The ivory, being fashioned to the required figure, is softened by dilute muriatic acid, which removes the hardening earth. In a brief memoir, Dr. Güterbock showed that the first idea of the preparation was not due either to the German or

Parisian individuals who had claimed the honour, as it was contained in an English work, published some time ago, under the title of "Useful Arts and Inventions."

STATISTICS.

Report on the Educational condition of the County of Rutland. By Mr. Wm. Langton, *on the part of the Manchester Statistical Society.*

It was stated that the Society, having previously examined the manufacturing districts where the population was dense, and the rate of increase rapid, had resolved to investigate an agricultural district where the population is scattered and nearly stationary. In comparing the counties of Rutland and Lancaster, the smallness of the parishes in the former appears striking, there being a parish church for every 400 inhabitants; the Roman catholic population is very small; there is no place of worship connected with that sect in the county: 2-7ths of the population belong to various sects of Protestant dissenters, the Wesleyan methodists preponderating,—the remaining 5-7th belong to the Established Church. The population of Rutlandshire was, in 1811, 16,383; in the decennial period between 1811 and 1821, it increased 13 per cent., but in the next decennial period the increase was only 5 per cent. In 1831 the population was—

Males	9,721
Females......	9,664
Total........	19,385

Taking the scholars of *all* ages,

1,117, or about 5·6 per cent. of the population, attend day and evening schools only.
1,922, or about 9·6 per cent. of the population, attend *both* day and Sunday schools.
1,274, or about 6·4 per cent. of the population, attend Sunday schools only.

4,313

Comparing these numbers with those derived from former investigations, the following are the results :—

Sunday Schools.

Manchester and Salford	17 per cent. of the population.
Rutlandshire	16
York	12
Liverpool	6

Day and Evening Schools.

York	17 per cent. of the population.
Rutlandshire	15
Liverpool	13
Manchester and Salford	10

There are as many endowed or charity schools in Rutland as there are parishes, but the majority are not superior to dame and common schools, and in two thirds of the number no books are provided. The teachers are generally of irreproachable character; and the dame-schools, in quiet, cleanliness, and orderly habits, afforded a very gratifying contrast to the schools of the same class in Manchester and Liverpool. Industrial education was very limited, but the girls were generally found sewing or knitting, and in many schools the boys learned to knit. The attendance of pupils is very irregular, as they are frequently detained to assist in farm labour at seed time and harvest. Out of 53 parishes, 46 have Sunday schools: the teachers are generally paid, and are most frequently masters and mistresses of day-schools. There is, however, a great want of systematic visitation. Good school-books are much wanted; and though the teachers are generally moral and respectable, they are not so systematically trained as to be fit to impart a good education.

Contributions to the Educational Statistics of Birmingham, by a Local Committee. Read by Francis Clark, *Esq.*

Mr. Clark stated, that a local committee had been formed to investigate the statistics of the borough of Birmingham, preparatory to the meeting of the British Association; but that in consequence of several unfortunate circumstances, and particularly the recent riots, which had engrossed the attention of the more active members, several heads of inquiry had been abandoned, and the information on others was meagre and imperfect. A Report on the general state of Education in Birmingham, prepared by Mr. Wood, agent to the Manchester Statistical Society, was found to be incomplete, and was, in consequence, withheld by the committee*. The Educational Returns presented by Mr. Clark comprised, 1. A return of the numbers, arrangement, and standing of the boys in the Free Grammar School of Edward VI.; 2. A return from the Blue Coat School; 3. A return from the Park-

* This return was prepared for the Birmingham Statistical Society of Education. It has been recently published in the London Statistical Journal, (April, 1840,) with the sanction of a General Meeting of the Society.

street Charity School; 4. A return from the Deaf and Dumb School.—1. *King Edward's School:* The number of boys educated is 444. Branch schools are fast coming into operation in various parts of the borough, for giving the children in the lower grades of the middle class a sound English education. The present income is about 4500*l.*, but in three years it will be doubled by the falling in of leases.—*The Blue Coat School*, for the education of children belonging to the Established Church. Income, 2715*l.* annually. Boys educated, 143; girls, 63; total, 206.—*Protestant Dissenting School*, in Park-street, for girls only. Income, 615*l.* 6*s.* 9*d.* Girls educated, 46. Products of labour, 7*l.* 5*s.* 1*d.*—*School for the Deaf and Dumb:* The present numbers are, boys, 22; girls, 25; total, 47. Of these, 37 were born deaf and dumb; 10 became so at a very early age from illness. In five cases, other members of the same family were similarly afflicted. To this report were appended some pathological and physiological remarks by M. De Puget, the master of the school, from which it appeared that the imperfection of the senses most frequently occurs in the offspring of marriages between first cousins and other near relations.—*Literary and Scientific Institutions:* The Philosophical contains about 400, the Mechanics 450, and the Athenæum 300 members. To this paper were added, an account of Lench's Trust, a charity for aged females; and an account of the number of inquests held during fourteen years. The average of inquests during the first seven years is 115, and during the last only 170, though the population has nearly doubled, and the use of dangerous machinery has increased in a still greater proportion. The average of accidents from machinery, in the first period, was 26, and in the second, 37.

A Report on the State of the Working Classes in part of Rutlandshire, by the Manchester Statistical Society, was read by W. R. Greg, *Esq.*

The Statistical Society of Manchester having completed and published an inquiry into the condition of the working classes in several large manufacturing towns in the north of England, were desirous of obtaining similar information with regard to some population differing in character and circumstances from those which had been previously examined. For this purpose they selected three parishes in Rutlandshire, (a purely agricultural county,) which they conceive may be assumed to afford a fair sample of the whole. The information thus obtained they have arranged in a series of tables, of which we shall briefly enumerate the most striking results. The parish of Branstown, which lies on the western side of the county, contiguous to Leicestershire, contains about 1400 acres, more than three-fourths of which are pasture land. It has a population of 1C2 families, comprising 425 individuals; but there is no resident clergyman, and no resident landlord possessing any extensive property. The parishes of Egleton and Hambleton (which have been classed together) are situated at a very short distance from Oakham, the county town, and contain about 2400

acres, which are about equally divided between arable and pasture. The chief part of both these parishes is the property of Mr. Finch, who resides upon his estates, and has the character of a kind landlord. The population consists of 100 families, comprising 479 individuals. The first three tables presented to the Section related to the dwellings of the population; and the account of them on the whole was satisfactory. The houses are low, never exceeding two stories; many of them are thatched, and nearly all are built of stone. To each a garden is attached, which is generally of sufficient dimensions to supply the family with vegetables. Forty per cent. of the dwellings in Branstown, and 51 per cent. in Egleton and Hambleton, are reported to be well furnished, which was very nearly the proportion found in Manchester and Salford. In Dukinfield the proportion well furnished was 61 per cent. The proportion reported to be comfortable were:—

In Branstown	50 per cent.
Egleton and Hambleton	65
Manchester	72
Dukinfield	95

The general appearance of the interior of the houses indicated thrifty poverty, and instances of squalid misery were very rare. Thirty-one per cent. of the houses in Egleton and Hambleton contained four rooms, and only 17 per cent. in Branstown. In respect to sleeping accommodations:—

In Egleton, 14 per cent. of families have more than three persons in a bed.

Branstown,	19	ditto	ditto.
Dukinfield,	33	ditto	ditto.
Bury,	35	ditto	ditto.

The rent of houses is low:—

	£	s.	d.
In Egleton, the average yearly rent is	£2	17	3
Branstown	3	0	0
Dukinfield	6	14	0
Manchester	7	11	8

The proportion of public-houses and beer-shops to the population is—

In Branstown	1 in 85.
Egleton	1 in 240.

The average weekly wages of the heads of families vary from 9*s.* 7*d.* to 10*s.* 8*d.*, and about one fourth of those under twenty-one are earning wages. The state of education in these parishes was rather satisfactory. The tables give the following results:—

	Per cent. can read.	Per cent. can write.
Branstown	75	44
Egleton, &c.	81	50
Manchester	50	Unknown.
Dukinfield	53	28

In Egleton, where there is a resident clergyman, the establishment has 82 per cent. of the population; and in Branstown, which is destitute of this advantage, only 73 per cent. In conclusion, the Committee report that their inquiries left a favourable impression as to the moral condition of these places. Swearing and drunkenness are far from common; and the general conduct of the people is marked by sobriety, frugality, and industry.

Contributions to the Commercial Statistics of Birmingham, prepared by a Local Committee. Presented by FRANCIS CLARK, *Esq.*

This paper includes returns from the Savings' Bank, the Assay Office, the Workhouse, and the Assessed Tax office, with a return of the steam power employed in the borough; and two others on the occupations and weekly wages of mechanics. The savings' bank report showed the satisfactory progress of this institution. It was established in 1827, and at the close of that year 980 accounts had been opened, and 33 closed, 2,337 deposits were made, to the amount of 10,612*l.*; the average of each deposit was 4*l.* 10*s.* 9*d.*, and of each account 10*l.* 16*s.* 9*d.* and the number of depositors 935. At the close of 1838, 1,597 accounts had been opened during that year, and 454 closed; 9,136 deposits entered on the books, amounting to 47,362*l.*; the average of each deposit was 4*l.* 17*s.* 4*d.*, and of each account 17*l.* 17*s.*, and the total number of depositors, 7,446. The amount of silver marked at the Assay Office, from its establishment in 1774, has been 4,011,997 ounces; and the weight of gold marked from the year 1825, when an Act was passed authorizing the assaying of gold manufactures, has been 27,167 ounces. The total amount of duty received is 105,851*l.* A curious fact was mentioned in this return; 25,000 gold wedding rings were assayed and marked at this office in last year. The workhouse return was complete for 19 years, and exhibited a classified report of the expenditure, with the average number of paupers, the proceeds of their labour, &c. The return of assessed taxes showed the amount collected in each year from 1817, and exhibited a very satisfactory improvement in the state of the town. The total amount of taxes collected in 1816 was about 36,000*l.*, and the sum paid in 1838, if calculated at the same rate, would have been considerably more than 50,000*l.* The steam power employed in Birmingham is at the present time 3,436 horses power, of which 2,155 horses power is employed in the metal trades of the town. The number of steam engines is 240, of which 65 are high pressure, and the remainder condensing engines. In the first 35 years after the introduction of steam power, only 42 engines were set to work; in the next 15 years 78 were erected, and in the last 8 years 120 have been established. The consumption of coal is estimated at 240 tons per day, and the number of persons employed at 5,200 males and 1,762 females. The return of occupations comprised 791 members of a Provident Institu-

tion, who were divided among no less than 110 different branches of trade, thus demonstrating the division of labour to an extent far beyond the calculation of political economists. Of the whole number, 56 per cent. were workers in the metals. The table of weekly wages showed the result of an inquiry into 662 cases, of which 479 were taken without choice from the members of the Provident Institution, mentioned in the last return. The averages were as follows: for boys 7 to 13 years of age 3*s.* 1*d.*, and for girls 2*s.* 4*d.* per week; from 14 to 20 years 5*s.* 9*d.* for males, and 5*s.* 2*d.* for females; for men and women 24*s.* 3*d.*, and 8*s.* per week. Reasons were assigned for the belief that these averages might be taken as a near approximation to the average wages of Birmingham mechanics.

Contributions to the Medical Statistics of Birmingham, by a Local Committee. Presented by FRANCIS CLARK, *Esq.*

Elaborate returns were received from the Town Infirmary, the General Hospital, the General Dispensary, and the Eye Infirmary; and also a return from the Superintendent Registrar of the Births, Marriages and Deaths during the last two years.

The medical institutions are of two different kinds, the first being a parochial infirmary for the relief of paupers, and the others being supported by subscriptions. The returns from the Town Infirmary by Mr. F. Ryland, were confined to the parish of Birmingham, there being none received from Edgbaston or Aston. They comprised,—1st, A return of the number of in-patients for the last seven years; 2nd, A return of the number of out-patients relieved in each quarter, for ten years, with the number who have received pecuniary relief; 3rd, Returns of the occupations and diseases of 9172 males, and 8774 females, out-patients; and, 4th, A return of the age, sex, and disease of 1518 cases of death among the out-patients.—The returns from the General Hospital by Mr. S. E. Bindlers, comprised,—1st, A return of 6133 cases, with the sex, disease and mortality; 2nd, A return of the number of in-patients and out-patients, and the amount of subscriptions received from the commencement, with the annual expenditure for the last ten years; and, 3rd, Four returns, showing the nature of the fractures in 943 cases occurring in the last two years, and statements of the results of the cases of paralysis, scirrhous and joint diseases, which had occurred in the same period.—The returns from the General Dispensary by Mr. F. Ryland, comprised the number of medical and midwifery cases, and of vaccinations, with the expenditure for the last ten years. It appeared that, during that period, 29,713 persons had received medical relief, 7,892 midwifery cases were attended, and 26,089 children vaccinated, at the cost of 13,105*l.* 1*s.* 3*d.*; giving an average of 4*s.* 9*d.* as the cost of the sick and midwifery cases.—The returns from the Eye Infirmary by Mr. Richard Middlemore, gave,—1st, The number, sex, and cost of the patients, from 1824 to March

1839; 2nd, The number of patients attended, from March 1828 to March 1835, with the diseases from which they suffered; and, 3rd, A return of Mr. Middlemore's patients for the year ending March 1839, giving the disease, sex, age, employment, and colour of the eyes of each patient, and likewise the result of treatment. The total number of patients reported is 23,554, and the expenditure 2,161*l.*; making the average cost of each case 1*s.* 9*d.*—With reference to the tables laid before the Section, the Committee regretted to find that the records of the various institutions did not afford all the information required to give value to such documents. In the practice of some of the surgeons the facts are all recorded, but the house-books do not contain them; so that the perfect returns only comprise a small portion of the cases. The last return produced by the Committee was the Superintendent Registrar's report, for two years, ending June 30, 1839. The births reported are 8,218, of which 347, or 4¼ per cent., were born out of wedlock. Of the 2,106 marriages, 1,854 were solemnized according to the rites of the Church of England.

Suggestions in favour of the Systematic Collection of the Statistics of Agriculture. By G. R. Porter, *Esq.*

After showing the amount of ignorance on this subject, which is so great, that to this day the public does not possess any authentic document, from which we can learn even the quantity of land under cultivation in any county of England, and that the only information available for further calculation is contained in the estimate of Mr. Couling, who gave evidence before a Committee of the House of Commons which was appointed in 1827 to inquire into the subject of emigration,—Mr. Porter explained the processes by which, in Belgium, Holland and France, a superior degree of information on agricultural statistics is collected. In a statement published in September, 1838, by the Prefect of the Department of the Eure, may be found a separate account for each canton, giving its population and superficial area, the distribution of the soil, the nature of the crops, the breadth of land appropriated to each kind, the total produce, the average price of each description of produce, the quantity used for seed, and the consumption by the native population. There are also given, the extent of land appropriated to the growth of wood, and the quantity lying fallow, the number and value of different kinds of animals reared and kept, the number slaughtered in the year, and the price of each kind of meat. In the course of a slight examination of this publication, comparing its results with such facts as we possess concerning the agriculture of our own country, some very striking differences appear, into the consideration of which it is not necessary to enter minutely on this occasion. It will, however, be interesting to state, that the produce of wheat throughout the department is not equal, on the average, to quite 18½ bushels to the English acre, and that the return obtained from the seed sown is not greater

than sevenfold,—results which may be pronounced by no means equal to those obtained by English farmers. The produce of barley, not much of which is grown in the department, does not exceed 17 bushels per acre; of oats not quite $20\frac{1}{2}$ bushels per acre are obtained.

Mr. Porter then entered at length into the arrangement and expense of the machinery necessary for the effective performance of corresponding labours in Britain, showing that a plan of comprehensive inquiry by public agents might be set in activity which should really be a means of saving large sums of money to this country in cases of deficient harvests and fluctuating prices of grain, and might have moreover a natural and necessary tendency to place the nation as far as possible, and for a long series of years beyond the probability of scarcity.

On the Criminal Statistics of England and Wales. *By* R. W. Rawson, *Esq.*

The author stated that although the numbers of the two sexes in European countries are nearly equal, the proponderance being somewhat on the side of the females, yet, both in England and France, the proportion of male to female criminals is about 4 to 1, and that result varies but slightly during several years. The average annual number of persons committed or bailed to take their trial during the last five years, was 22,174; the difference between the highest and lowest annual number during the period was 14 per cent. Taking the twenty principal offences in their relative order, according to the number of persons annually committed for each, they will stand thus:—

1.	Simple larceny	12,303
2.	Stealing from the person	1,539
3.	Housebreaking and burglary united	1,007
4.	Stealing by servants	955
5.	Assaults	756
6.	Receiving stolen goods	683
7.	Riot and breach of the peace	607
8.	Resisting or refusing to aid peace-officers	579
9.	Frauds and attempts to defraud	425
10.	Robbery and attempts at robbery	392
11.	Uttering counterfeit coin	318
12.	Sheep-stealing	292
13.	Embezzlement	262
14.	Manslaughter	209
15.	Rape and attempts to ravish	188
16.	Stealing from houses to the value of *5l.*	178
17.	Stealing of fixtures, trees, and shrubs	163
18.	Horse-stealing	155
19.	Poaching	153
20.	Keeping disorderly houses	145

The ages of the offenders are divided in the official tables into eight periods, and it is a curious fact that the greatest variation during the last three years, in the proportion of any class of criminals at the same period of life, has not exceeded ½ per cent.

Centesimal Proportion of Offenders at each Age.

	1836.	1837.	1838.	Greatest Difference.
Under 12 years....	1·84	1·52	1·58	0·32
From 12 to 16....	9·71	9·72	9·92	0·21
17 „ 21....	29·03	29·23	29·13	0·20
22 „ 30....	31·42	31·74	31·24	0·50
31 „ 40....	14·43	14·56	14·75	0·32
41 „ 50....	6·76	6·65	7·02	0·26
51 „ 60....	3·33	3·24	3·00	0·33
Above 60	1·40	1·55	1·58	0·18
Not ascertained ..	2·08	1·79	1·78	
Total....	100·	100·	100·	

The average population of England and Wales, during the five years under examination, may be assumed to be 15,026,447.

Adopting this total, and the proportions above given, the numbers existing at each period of life will be as follows:—

Under 16	5,875,340
From 16 to 20	1,502,644
21 „ 30	2,374,178
31 „ 40	1,788,147
41 „ 50	1,412,486
51 „ 60	991,745
Above 60	1,081,907
Total	15,026,447

The following, therefore, is the proportion of offenders annually committed, to the population at each interval of age, adding to the number under 15 one fifth of the number between 16 and 21, and taking away from that between 21 and 30 one tenth, in order to equalize the periods of comparison :—

	No. of Offences annually committed on the average of the five years.	Proportion of offences to the Population.	
Under 17	2,539	One offence in 2,432	individuals.
From 17 to 21	6,468	232	„
22 „ 30	6,997	305	„
31 „ 40	3,184	561	„
41 „ 50	1,501	941	„
51 „ 60	703	1,410	„
Above 60	319	3,391	„

Further it appeared, 1st, that crime prevails to the greatest extent in large towns ;—2nd, the difference between manufacturing and agricultural counties, in which the influence of large towns is not much felt,

is not very great;—3rd, crime is very much below the average in mining counties (Cornwall, Glamorgan, Durham, Northumberland);—4th, and it is still less frequent in Wales and the mountainous districts of the north of England. In all the mining counties, widely separated as they are, the proportion of criminals, according to these tables, is less than half the average; and Derbyshire, in which much mining is carried on, but which was placed by Mr. Rawson among the manufacturing counties, scarcely exceeds half the average.

On Academic Statistics, showing the proportion of Students in the University of Oxford who proceed on to Degrees. By the Rev. Professor Powell, *F.R.S.*

University of Oxford.

Year.	Number Matriculated.	Passed Examination.	Obtained Honours.			Obtained Degrees.					
			Classical honours.	Mathem. honours.	Both.	B.A.	M.A.	In Civil Law (ordinary).	In Civil Law (honorary).	Medicine.	Divinity.
1831	387	279	107	22	15	228	178	9	6	2	16
1832	377	275	104	21	17	269	175	5	8	1	11
1833	384	291	135	25	16	292	186	4	[B. Assoc.] 1	8	13
1834	360	292	120	21	15	304	207	11	76	7	16
1835	369	292	105	22	8	272	173	20	[Installation] 6	5	1
1836	369	275	121	28	20	298	200	7	1	6	21
1837	421	261	124	24	18	246	161	6	1	2	14
1838	393	274	105	24	10	264	181	13	1	10	12
Mean	382	279	115	23	15	271	183	9		5	13

On these data we may observe:

1. The proportion of those who enter different professions cannot be estimated. The degrees in civil law are only taken either for practising in Doctors' Commons, or by the statutes of particular colleges. Those in divinity, chiefly by those who have preferment in the church; while the great body of those who take orders have only the degrees of B.A. or M.A. In medicine alone can the proportion be estimated: which is to those who pass the examination in arts (a necessary preliminary) as 1 : 55.8.

2. The difference between the number who are matriculated and those who pass the examination, is occasioned, 1st, by those who fail in the examination; 2nd, those who, from various causes, do not remain in the University: such as being directly or indirectly sent away on account of irregular conduct, &c. The ratio of this difference to the number who pass, or of irregular to regular men, will be found 1 : 2.67.

3. The mathematical honours (which imply all degrees of attainment in mathematical science, from the highest to a knowledge of somewhat more than the mere letter of four books of Euclid,) form the only public test of any cultivation of science in the University. The

proportion, then, of those who evince any, even the smallest, knowledge of science, to those who pass the examination, is 1:12.

Notice of the Progress of the Inquiries made by the Committee instituted at the meeting of the British Association in Newcastle, when the sum of 50l. was placed at the disposal of Mr. Cargill, Mr. Wharton, Mr. Buddle, Mr. Forster, Mr. Wilson, and Mr. Johnston, for the purpose of making inquiries into the Statistics of the Mining Districts of Northumberland, Durham, and Yorkshire. Presented by W. L. WHARTON, *Esq.*

(The subject is still under investigation.)

Account of the Circulating Libraries in the Borough of Kingston-upon-Hull. By the Manchester Statistical Society.

Circulating Libraries in the borough of Kingston-upon-Hull, may be ranged under the following heads:—1. Public Subscription Libraries. 2. Libraries attached to Public Institutions. 3. Congregational Libraries. 4. Libraries attached to Sunday Schools. 5. Private Circulating Libraries.

These public subscription libraries contain an extensive assortment of works in every department of literature. There are four, containing 25,671 volumes; of which, 2,537, or 9·88 per cent., are Theology and Ecclesiastical History; 2,674, or 10·41 per cent., are Jurisprudence and Political Economy; 7,549, or 29·41 per cent., are History and Biography; 9,566, or 37·27 per cent., are works on the Arts, Sciences, and general Literature; and 3,345, or 13·03 per cent., are Novels, Romances, and works of imagination. The circulation is 102,180 volumes per annum, affording an average of 126 volumes annually to each member. There are four libraries connected with *public institutions*, and they contain 2,920 volumes, of which, 467, or 15·99 per cent. are works in Theology and Ecclesiastical History; 26, or ·89 per cent., are on Jurisprudence and Political Economy; 1,016, or 34·80 per cent., are History and Biography; 1,397, or 47·84 per cent., are Arts, Sciences, and general Literature, and 14, or ·48 per cent., are Novels, Romances, and works of imagination. The Library of the Mechanics' Institution contains 2,260 volumes, the average annual circulation being 17,992 exhibiting an average reading of 52 volumes per annum to each subscriber.

There are ten congregational libraries attached to churches or chapels, and designed to promote the religious instruction of the congregation. In these libraries are 2,994 volumes, which are, with scarcely an exception, of a religious character. The average circulation is 10,088 volumes per annum. The number of persons having access to these libraries not being in all cases ascertainable, no estimate of the average number of volumes to each can be made. There are 28 libraries attached to Sunday schools, which have 5,655 volumes, exclusively of a religious tendency, with an annual circulation of 48,942 volumes,

which takes place chiefly among the senior scholars and their teachers. Nearly the whole of the Sunday school libraries contain a variety of works of fiction, having, however, a religious object. There are 11 private circulating libraries, having 17,474 volumes, 8 of which, or ·04 per cent., are works in Theology and Ecclesiastical History; 9 or ·05 per cent., Jurisprudence and Political Economy; 220, or 1·26 per cent., History and Biography; 26, or ·14 per cent., Arts, Sciences, and general Literature; and 17,211, or 98·51 per cent., Novels, Romances, and works of imagination.

On the condition of the Working Classes in the City of Bristol. (*Report of a Committee presented by* C. B. Fripp, *Esq.*)

The following is an Analysis of this Report:—

Analysis of the Inquiry into the Condition of the Working Classes in the City of Bristol.

Number of Houses examined		3028
Containing Families	per ho. 1·97 ...	5981
Consisting of Persons	per ho. 6·84; per fam. 3·46 ...	20717
Heads of Families with or without children:—		
Men ... Married, 3880; single or widowers, 703.	4583	
Women... Married, 3880; single or widows, 1398.	5278	
Children...Boys, 5363; girls, 5493. (2·82 per fam.)	10856	
		20717
Nation—English, 5220; Irish, 501; Welsh, 170; Scotch, 15. ...	5906	
French, 5; Italian, 6; Dutch, 5; German, 5; Prussian, 2; Swiss, 1; East and West Indian, 2; American, 1; not ascertained, 48.	75	5981
Families having children, 3846; not having children, 2135. ...		5981
occupying airy apartments	3569	
apartments close and confined	2412	
Families consisting of		5981
Single persons (unmarried, widows, or widowers, 1163; two persons, 1298; three persons, 900; four persons, 792; five persons, 691; six persons, 470; seven persons, 269; eight or more persons, 308. ...		5981
Families occupying part of a room only, 556; one room only, 2244; two rooms only, 1439; three or more rooms, 1742.		5981
Families having sufficient cupboards or shelves, 3688; some, but deficient 1421; without any, 872.		5981
Families having religious books: Bible and Prayerbook only, or both, 3430; having other books or tracts, or parts of some, 947; not having any books or tracts (including 2 not ascertained) 1604.		5981
Families having prints of some kind on the walls, 3030; not having any, 2938; not ascertained, 13.		5981
Families clean and respectable, 3610; dirty and disreputable, 1095; in considerable distress, 660; condition not ascertained, 616.		5981
Heads of Families depositors in savings' banks, or members of benefit societies or trade clubs, 940; not depositors, nor belonging to any benefit society, &c. 4973; not ascertained, 68.		5981

Heads of Families who can read and write (more or less), 5122; only read, 2523; total who can read, 7645; unable to read or write, 2204; not ascertained, 12. (Men 4,583, Women 5,278)	9861
Men who can use carpenters' tools so as to mend their own furniture (on their own statement), 2703; who cannot use tools, 1880.	4583
Women who can sew and wash, 5156, (of whom can also knit 297); who cannot sew or wash, 122.	5278

Rents, &c.

Families renting house or apartments from owners, 3298; tenants, 2666; occupying their own houses, 13; apartments free, 4.	5981

Average rent paid by

		£.	s.	d.	
1799	Families, for 1 room unfurnished, ...	0	1	3¾	per week.
4	... ...		free		—
943	... ... 2 rooms	0	1	11¾	—
790	... ... 3 rooms	0	2	5¾	—
632	... ... 1 room furnished ...	0	2	0¾	—
10	... ... 2 rooms	0	2	10¾	—
1156	... ... houses (under 20*l.*) ...	9	9	8	per ann.
59	... ... do. (20*l.* and above).				
588	... ... not ascertained.				
5981					

		£.	s.	d.	
Of the houses the lowest rent was		3	0	0	per ann.
the average rent of 47 not exceeding 5*l.* was		4	10	9	—
	561 above 5*l.* and under 10*l.*	7	17	11	—
therefore	608 below 10*l.*	7	12	9	—
	548 of 10*l.* and below 20*l.*	12	5	3	—
Rent 20*l.* and above	59				
	1215				

Children, &c.	Boys.	Girls.
Of the age of 1 year and under	398	443
2 years	411	457
3 years	339	366
4 years	333	338
5 years	304	313
6 years	247	265
7 years	338	276
8 years	237	279
9 years	274	279
10 years	276	264
11 years	206	218
12 years	278	258
13 years	209	210
14 years	294	233
Above 14 years	1219	1294
	5363 +	5493 = 10856

Of whom are healthy	10085	
unhealthy (1-14th)	771	
		10856
Children above 7 years old, sleeping in same room with parents, or both sexes in same room	...	4752

Children brought up to trade or useful occupations		2687	
not so brought up (above 14 years old)		731	
			3418
Girls who can sew and wash		1702	
sew only		1350	
cannot sew, wash, or knit (old enough)		74	
too young, or not accounted for		2367	
			5493
Children at School:			
Not above 3 years of age	120		
From 3 to 14 years old	3394		
Above 14 years old	222		
		3736	
Children not at School:			
Not above 3 years of age	2294		
From 3 to 14 years old	2535		
Above 14 years old	2291		
		7120	
			10856
Children stated by their parents to be able to read and write	2010		
able to read only	3934		
		5944	
Children unable to read or write:			
Under 7 years of age	3603		
Above	1309		
		4912	
			10856
Children able to repeat the Lord's prayer		6504	
not able, or too young		4352	
			10856

Payments by Scholars:

Gratis (chiefly Sunday scholars), 1425; at $0\frac{1}{2}d.$ per week, 6; at 1*d.*, 715; at $1\frac{1}{2}d.$, 181; at 2*d.*, 650; at 3*d.*, 397; at 4*d.*, 165; at 5*d.*, 3; at 6*d.*, 85; at 7*d.*, 14; at 8*d.*, 36; at 9*d.* to 1*s.*, 27; paid for by friends, 11; not ascertained, 21. 3736

Religious Profession.

Church of England, 4547; Roman Catholics, 489; Methodists, 223; other Dissenters, 589; Jews, 5; without any profession, 81; not ascertained, 47. ... ... Heads of Families 5981

Bodily Complaints.

Cripples, 18; spinal deformities and accidents, 24; paralytic, fits, Vitus's dance, &c., 48; dumb, 6; blind, 12; idiots and insane, 21. 129

Small Pox.*

Natural Pox, 1632; Vaccinated, 3535; Inoculated, 93; neither, 1102; caught Small Pox after Vaccination, 17. 6379

Houses.

With drains or sewers, 2398; without drains, or stopped, 630. 3028

With privies, 2451; without, or very bad, 577. 3028

With a good supply of water, 1724; without, or very bad or deficient, 1304. 3028

* The inquiry relative to the small-pox was not included until several parishes had been gone through, and applies only to 6362 children out of the 10,856. There can be no doubt, however, that the results are nearly the same as would have been afforded by a wider examination.

MECHANICAL SCIENCE.

On the most Economical Proportion of Power to Tonnage in Steam Vessels. By J. Scott Russell, *Esq.*

It is a subject of anxious inquiry with every proprietor and constructor of steam vessels, when about to construct a new steam vessel for a given station, What is the best amount of power to place in my ship, so as to accomplish in the highest degree economy, rapidity, and regularity? The inquiry is one accompanied with difficulties. If, on the one hand, a large cargo is desired, which is generally the case, the power of the engine is made small, for the purpose of occupying small bulk, and consuming little coal; and although less velocity is then acquired than with greater power, still less fuel is consumed with a small engine than with a large one, and thus it is supposed that greater economy is effected. This maxim, of a small proportion of power to tonnage, is one on which many companies have long continued to act. In other cases, where velocity is absolutely required, a much larger proportion of power has been employed, and of course, by a large power, there is a much greater consumption of fuel than by a small one in a given time; and not only so, but it is well known that this additional consumption is much greater than in the proportion of the velocity gained; so much so, that a consumption of four times as much fuel will not give more than about double the velocity. Thus it has appeared that the use of very great powers, and great expenditure of fuel, has been made with only a very slight increase of velocity. All this, the usual reasoning on the subject, goes to prove the value of employing a small proportion of power to tonnage for economy. The advantage of low powers and of low velocities, in point of economy, would thus appear to be established; but this apparent advantage in theory has not been realized; on the contrary, cases had been mentioned, at the Bristol meeting of the Association, in which it was found, that by a gradual increase of power in the same vessel, while the speed was increased, the consumption of fuel on the whole had diminished. Mr. Russell, considering this subject worthy of further examination, examined the books of the expenditure of fuel in the steam vessels of several companies, and found that they were aware that they had, on the whole, saved money by using high powers of steam and high velocities, instead of low ones. This fact he had examined carefully, and had arrived at a remarkable general result, which appeared to him quite new, and to be of very great value at the present moment, when such important interests were involved in the successful extension of deep sea navigation. The general principle at which Mr. Russell had arrived, was this:—*That in a voyage by a steam vessel in the open sea, exposed of course to adverse winds, there is a certain high velocity and higher proportion of power, which may be accomplished with less expenditure of fuel and of steam, than at a lower*

speed with less power. This principle he then proceeded to prove, and to illustrate by the following case, in which the same vessel is taken with different powers of engine, and the result, as regards expenditure of fuel, determined first arithmetically, and then by a general formula, which will enable any one to determine any particular case:—

FAIR WEATHER.

1,200 tons, 400 horse-power, 9 miles an hour, 216 miles per day, 1 ton of coals per hour.—2,160 miles in 10 days, 240 tons of coal.

1,200 tons, 500 horse-power, 10 miles an hour, 240 miles per day, $1\frac{1}{4}$ ton of coal per hour.—2,160 miles in 9 days, 270 tons of coal.

ADVERSE WEATHER.

1,200 tons, 400 horse-power, 5 miles per hour, 120 miles per day, 1 ton of coals per hour.—2,160 miles in 18 days, 436 tons of coal.

1,200 tons, 500 horse-power, $6\frac{3}{4}$ miles per hour, 162 miles per day, $1\frac{1}{4}$ ton of coals per hour.—2,160 miles in 13 1-5th days, 395 tons of coal.

General Formula.—Let v represent the velocity of a given steam vessel in a favourable voyage; v' the same vessel in an unfavourable voyage; v'' a vessel, higher power, favourable voyage; v''' same in unfavourable voyage; p the power of the former vessel; p' latter vessel:—

$$\therefore \sqrt{v''' - (v - v')} = v''' = \sqrt{v}\sqrt{\frac{p'}{p} - (v - v')} \quad v\ \frac{p'}{p} = \sqrt{\frac{v'' - (v - v')}{v'}}$$

in the case of equal expense, whence the highest proportion of power that will be economical in fuel may be at once obtained.

Experiments to ascertain the power of different species of Wood to resist a force tending to crush them. By E. Hodgkinson, *Esq.*

The specimens upon which trials have been made were turned into right cylinders, about one inch in diameter, and two inches long. The apparatus used to crush them has been described by Mr. Hodgkinson in an account of his experiments on cast iron, published in the Transactions of the Association. The crushing surfaces were perfectly parallel, and the body to be crushed had its ends bedded firmly against them, the force being applied in the direction of the fibres. The specimens broke by sliding off in a constant angle, dependent on the nature of the material, as the writer had found to be the case in cast iron and other bodies, showing that the strength in any particular species of bodies is directly as the area of the section. Great discrepancies were found when the woods were in different degrees of dryness—wet timber, though felled for a considerable time, bearing, in some instances, less than one half of what was borne when dry. These experiments

were made at the expense of Mr. William Fairbairn. The following were some of the results:—

Description of wood.	No. of experiments made.	Mean force, per square inch, which crushed the specimen.
Yellow Pine	3	5375 lb.
Cedar	3	5674
Red Deal	3	5748
Poplar, not quite dry	3	3107
" dried two months, (length 1 inch)	1	5124
Larch, green	3	3201
" dried one month, (length 1 inch)	1	5568
Plum-tree, wet, though felled two years	3	3654
Plum-tree, dried two months	3	8241
Birch, green	3	3297
" dried two months, (length 1 inch)	3	6402
Sycamore	3	7082
Ash	3	8683
" dried two months, (length 1 inch)	1	9363
English oak	3	6484
" dried two months, (length 8 inches)	2	9509
Spanish mahogany	3	8198
Box-tree	7	9771

The woods above (except the Poplar, Larch, Plum, Birch, and Oak,) were all moderately dry; and where it is stated that they had been dried for any particular time, it is to be understood, that specimens prepared, and not used when the first experiments were made, were kept in a warm, dry place, during the time mentioned, and afterwards re-measured and crushed.

Experiments upon the effects of Weights acting for an indefinite time upon bars of Iron. By WM. FAIRBAIRN, *Esq.*

The experiments, of which the present is a notice, were commenced by Mr. Fairbairn in March, 1837, when a number of bars of Coedtalon iron cast from the same model, 5 feet long and 1 inch square, were placed horizontally on props 4 feet 6 inches asunder, and had different weights, as $2\frac{1}{2}$, 3, $3\frac{1}{2}$, and 4 cwt., laid upon the middle of each, the last weight being within a few pounds of the breaking weight. The intention was to ascertain what effect would arise from each of these weights lying constantly upon the bars. The results are, 1st. The bars are still bearing the loads, and apparently may do so for many years. 2nd. The deflections, which are frequently measured, the temperature being observed at the time, are constantly increasing, though in a decreasing ratio,—a fact which shows that, though cast

iron may be safely loaded far beyond what has hitherto been deemed prudent, still it is extremely probable that the bars are advancing, by however slow degrees, to ultimate destruction.

On Paving Roads and Streets with blocks of Wood, placed in a vertical position. By JOHN ISAAC HAWKINS, *Esq.*

The subject, the author observed, has latterly become one of considerable interest. Although seven patents have been taken out in this country within less than twelve months, there is no specimen of the pavement calculated to afford the means of forming a sound public opinion on the subject. He had attentively watched, from 1827 to 1831, the effect of much travelling over wooden pavement, well executed, in the principal thoroughfare of Vienna, and observed that it appeared to wear away less than any other kind of paving material whatever. In this opinion he was confirmed by inquiries which he made relative to the condition of a piece of wooden pavement laid about three years in the Broadway of New York; and he had been informed that a stone of near twenty tons' weight had been drawn on a carriage over it without appearing to make the least impression. From these circumstances he considered that roads formed of sound wood, with the grain vertical, might be made so even as to constitute a sort of universal railway, on which carriages might be drawn by a small proportion of horse-power, and on which steam carriages might run as safely and almost as fast as on railways. The directions to be attended to in the formation of efficient and durable roads on this principle, which the author gave, were comprised under the following heads:—1. The wood must be chosen from the heart of sound trees. Larch and other resinous firs offer excellent materials at moderate prices. 2. The blocks, which are to be laid contiguously, must be cut to an exact gauge, so as to fit closely and evenly together, and no block must be higher than another. 3. The depth of the blocks should be at least that of a breadth and a half, a firm lateral support being found necessary to stability. Each block, when rectangular, is supported by four others, and when formed into hexagonal prisms, which appears to be preferred, each block is supported by the six surrounding ones. The hexagonal prism being found to afford the greatest quantity of wood from a tree when the diameter of the prism is as large as can be cut out of the whole diameter of the tree, that figure is generally adopted, and has been fairly tested by experience. 4. The blocks must be laid on a bed firmly made with gravel, shingle, hard rubbish, or other material, well rammed down, and made even, previously to laying the blocks. 5. A thin layer of only half an inch of fine gravel must be spread evenly over the levelled surface at the time of laying the blocks, to favour their adjustment. 6. The blocks must be laid so as to present an even upper surface before they are rammed, in order that the ultimate making them level shall not depend so much on the effects of the rammer as on the evenness of the bed. It is essential that the blocks be cut from dry wood, and used soon after being cut, lest their figure vary by warping.

On the Marquis of Tweeddale's Patent Brick and Tile Machine. By G. COTTAM, *Esq.*

The first process by this machine is to make a continuous sheet of well-pressed clay, of the proper breadth and thickness. This is then cut into the required lengths. It moulds at the rate of 24 bricks per minute, or 1,440 per hour, and, in a brickmaker's day of 16 hours, would produce 23,040 per day; and, in consequeuce of the compression which the clay undergoes, the bricks do not require one third of the time to dry them that the hand-made bricks do. The tile machine is a modification of that for bricks. In each case the clay is made to pass between two rollers, from whence it is brought out in a thin flat cake, and is cut to the requisite width by two wires. It is then conveyed, by an endless web, under other rollers, and, by a simple contrivance, the tiles are cut to the required size, the web further conveying them on to the shelves, from whence they are taken to be burnt. By a modification of the machine, the drain-tile and the pan-tile can be manufactured with equal facility.

Description of a new Railway Wheel. By G. COTTAM, *Esq.*

The wheels suggested are made on the following principles:—1st. They are wholly of wrought iron, so welded together, that, independent of screws, rivets, or any other kind of fastening, they form one piece with the spokes. 2nd. The spokes of the wheels are placed diagonally, and act as trusses, thereby giving the greatest possible support to the rim, or tire, and, at the same time, being in the best position for resisting lateral pressure. 3rd. Iron in a state of tension or compression, as is usually the case with the tires of wheels, is easily broken by sudden shocks, or by vibratory action. The wheels in question are so constructed, that the fibres of the iron employed are neither compressed nor stretched, but remain in their natural condition. 4. The strength of iron being as the square of its depth, then the flanged tires of these wheels, which offer sections twice as deep, are, consequently, four times as strong as those of any wheels at present in use. This increase of strength is attributable solely to the peculiarity of their construction, and not to any increase in the weight of the material. 5th. The spokes strike the air edgewise, and thus offer the least possible resistance. Wheels where the spokes present a flat surface, may be said to act as blowing machines, and, as such, require a greater propelling power. 6th. These wheels, by simply varying the curve of their spokes, become either rigid or flexible, or, in other words, they may be made to any degree of elasticity. 7th. When worn by friction, the rims or tires may be turned down, and have hoops of railway tire shrunk on them. These wheels are very strong and durable, and more advantageous than those of other constructions.

Notice of an Apparatus for Use in Working Railways. By Dr. LARDNER.

From various circumstances which he had observed, the author was of opinion that the rails were often not so perfect as they ought to be, and he had hence thought it advisable to make some experiments to ascertain their rate of deflexion, his object being to form an idea of their relative merit of surfaces. He made use of a truck, with wheels without flanges, and perfectly cylindrical, and on which a platform was placed. An iron tube crossed this, terminating at each end at right angles, and into this was introduced mercury, so that it was in fact a mercurial level. Into each of the mercurial columns was introduced a piston rod, to the top of which a pencil was attached, which on any incorrectness of the line described a curve on a sheet of paper, and the ordinate of this curve gave the variation of the rail. He had tried this on several parts of a line, where it gave a continual variation in the curve, amounting even from three to five inches. The instrument was checked so as to show that this curve was the real representation of the line, and, being simple and easily applied, would no doubt be found useful to contractors on new lines of rails.

On a new Rotatory Steam-engine. By Mr. GOSSAGE.

The object of this communication was to make known to the members present in the mechanical section, the peculiarities and advantages of an uncommonly simple form of rotatory steam engine, which had been brought into actual use under the title of the Stoke Prior Engine. The construction can scarcely be properly understood, except by a very full description and drawings.

Description of a Machine for cutting the teeth of Bevel Wheels. By Mr. DAVIES.

In consequence of the importance of the subject the attention of practical and scientific men has been for some time devoted to the inquiry, and at the Liverpool meeting of the Association Prof. Willis communicated the results of his investigations. The mechanical invention now explained was to provide the means by which any form when determined, could be accurately obtained, but was applied more particularly to the formation of the teeth of bevelled wheels. In constructing this apparatus, Mr. Davies availed himself of the well-known planing machine, which provides the means of bringing any piece of work attached to its moving table in contact with the cutting tool of the machine, the cuttings thus produced being in lines parallel with the bed. The arrangement for the machine provides for the wheel being caused to have a revolving movement either in a horizontal or a vertical plane, the combination of these two movements being similar to that of a universal joint. A lever or guide-rod is attached to the frame at one end, and at the other it is confined by a slit, which it fits exactly. This slit is formed in a vertical piece of metal which is attached to the moveable

table of the machine. Motion is given to this guide by means of a screw, when it describes the exact curve of the slit, and this movement is communicated to the bevelled wheel through the frame to which it is attached. As the part of this guide-rod which, by its traversing, influences the form, is much more distant from the centre of motion than the wheel, any error in the form of the slit will be diminished on the wheel in direct proportion to these differences. The only precautions requisite in using this machine are to ensure that the centre of the cone, which would be formed by the extension of the bevelled wheel, is true with the centre of the bearings; and that the cutter be so placed, that in the traversing of the machine the centre point shall constantly approach the cutting point of the tool.

On the application of Anthracite Coal to the Blast Furnace, Steam-engine boiler, and Smith's fire, at the Gwendraeth Ironworks near Caermarthen. By Mr. Player.

The inconvenience of the fire choking for a long time baffled the experiments made on the subject, but it was at last obviated by heating the coal before it reaches the fire. This was accomplished by supplying it, without any mixture of coke or bituminous coal, through a perpendicular chamber placed centrally on the top of the boiler, with an opening about 20 inches in diameter immediately over the fire-place. In passing through this chamber, by its contact with the plates, the coal acquires considerable heat, and descending by its own gravity, as the fire consumes beneath, replaces what has been burnt, by which means a regular supply of fuel is furnished, fit for immediate and complete ignition. Another inconvenience is also thus avoided, as fresh coal thrown upon the fire abstracts a quantity of heat from the fuel already in ignition, and checks the generation of steam. The fire is never meddled with; there are no fire drawers; there is no current of cold air passing through the flues, and a very small amount only of draught is required. One engine worked 72 hours consecutively, during which time the grate neither choked nor clinkered; nor was a bar used for the fire, or did there remain any considerable result in ashes. The coal was, in this instance, entirely anthracite, (small, but not powdery,) and tipped into the feeding chamber once every four hours. Water was also kept in the ash-pit, the steam from which being decomposed by passing through the fire, the gas forms a jet of flame, creating another active source of power. On these works, there are in action upon this principle, five smith's fires, the tool-maker's fire being blown by a 30-inch bellows only, whilst with this the largest squaring edges for the masons are made with ease. The coal is supplied through an upright brick flue, about three feet six inches high, two feet six inches long, and nine inches wide. The foundry has a similar arrangement, with merely the addition of a flue to take off the flame, the blast being cold, and worked by a small water-wheel, and by which iron is re-melted, running very fluid, and yielding an excellent quality. An oven has also been built for the use of the workmen, heated only with small culm, which succeeds admirably.

On Warming and Ventilating. By Mr. Jeffries.

In this communication the author gave a description of a new Pneumatic Stove.

Account of a Method of Filtering Liquids. By Mr. Beart.

The principle of the process recommended by the author is to use a perforated packed piston in a cylindrical or other vessel, so that on raising the piston a partial vacuum is formed, and the liquid is urged to filter through the material used in the construction of the piston, by the pressure of the atmosphere, added to the weight of the column of liquid. The application of this to the making of coffee and other infusions was explained.

Remarks on Bridge Architecture. By Mr. Dredge.

A new Secret Lock, without a key, by Mr. Benge, was exhibited.

A model, sent by Mr. Hamilton, of Edinburgh, was explained, of a method by which the resistance caused by the pressure of the wind against the valves of the organ can be overcome, thereby permitting the largest pipes to be played by the fingers with facility, and also rendering the movement of pedal keys and valves more smooth.

On the Scientific principles, geometrical forms and proportions, and the constructive skill manifested in the execution of the Cathedrals and other large Churches of the Middle Ages; with incidental remarks on the symmetry, unity and harmony of ancient ecclesiastical Architecture. Illustrated by numerous Drawings. By John Britton, *F.S.A., &c.*

On Percussion Boring of Tunnels. By C. Vignoles, *F.R.S.*

On the method of rolling Dovetailed Grooves for Railways. By Wm. Carpmael.

On a new construction of Wooden Railway Wheels. By Thomas Parkin.

The author, after stating the imperfections and liabilities to accidental breaking which attend iron wheels of the ordinary construction, endeavoured to prove that wheels properly constructed, chiefly of wood, will be much more secure than those of iron. The author stated that a specimen sent for inspection could not be broken in actual service, however severe. Among other advantages attending the plan he advocated, Mr. Parkin mentions the facility of widening the tire of the wheels, the better 'bite' of the locomotive on the rails, and the diminished wearing of the rails themselves.

On Railway Foundations. By Thomas Parkin.

The author, after noticing the various modes which have been adopted for securing railway foundations, expresses his surprise that 'concrete,' often proved to be very efficient for foundations of buildings, even in swampy ground, has scarcely been thought of for railways. He gives an example where 500 yards of rails were laid down more than two years ago on a colliery railway, in South Wales, where the traffic is both heavy and great, and though in constant use ever since, the level and gauge have remained perfect, and no expense has been incurred in repairs.

From this and other examples cited on the Southampton and Greenwich Railways, Mr. Parkin recommends the use of continuous beams of wood laid upon and imbedded in concrete, the surface up to the top of the wood being covered with a cement made of boiled gas-tar, and sand, about an inch thick.

On the Evaporative Calorific Powers of Fuel. By Dr. Ure.

On methods adapted to increase the security and extend the advantages upon Railroads. By W. J. Curtis.

On a new method of forming Fuel. By Stephen Geary.

On a new Kitchen Range, with a Model. By Mr. King.

On folding Plates in Books and Maps for the Pocket. By John Isaac Hawkins.

In folding plates in books or maps for the pocket, it is usual, first to reduce the height of the plate or map by a few horizontal folds, so as to correspond with the height of the book or of the pocket-case, and then reduce the width by as many vertical folds as will bring it within the width of the book or pocket-case.

Mr. Hawkins's plan is, to reverse this order, and first to fold the maps or plates in the width, usually the greater number of folds, laid alternately forwards and backwards, and then to fold the length into as few folds as convenient.

By this plan of folding, the map or plate can be referred to in parts conveniently, and the paper is not liable to be torn, advantages altogether sacrificed by the ordinary process.

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TO

REPORTS ON THE STATE OF SCIENCE.

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TO

MISCELLANEOUS COMMUNICATIONS TO THE SECTIONS.

THE END.

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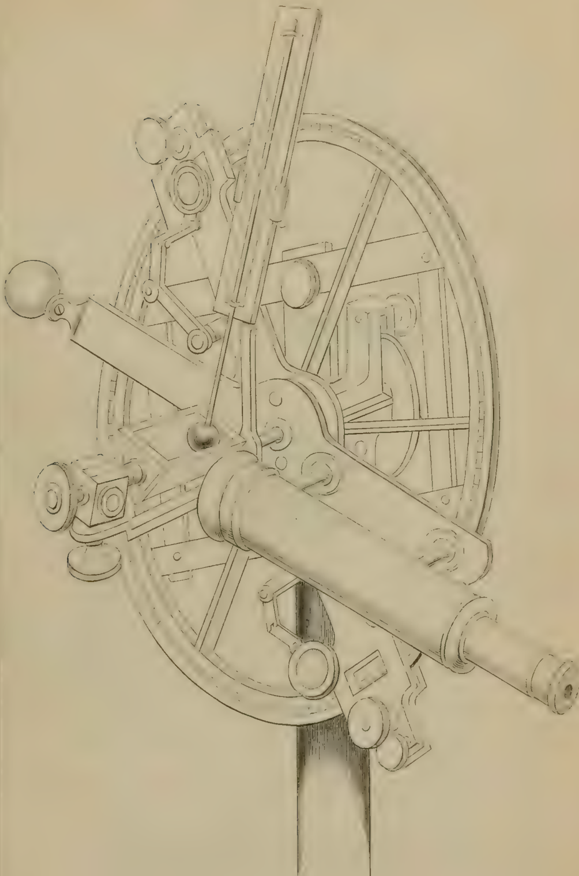

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Diameter of Circle - 10 Inches.

Solar Spectrum Oil of Aniseed.

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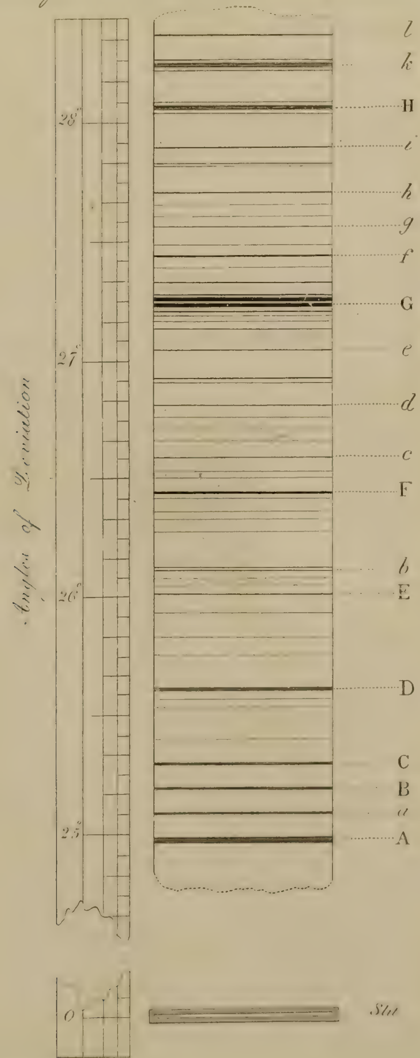

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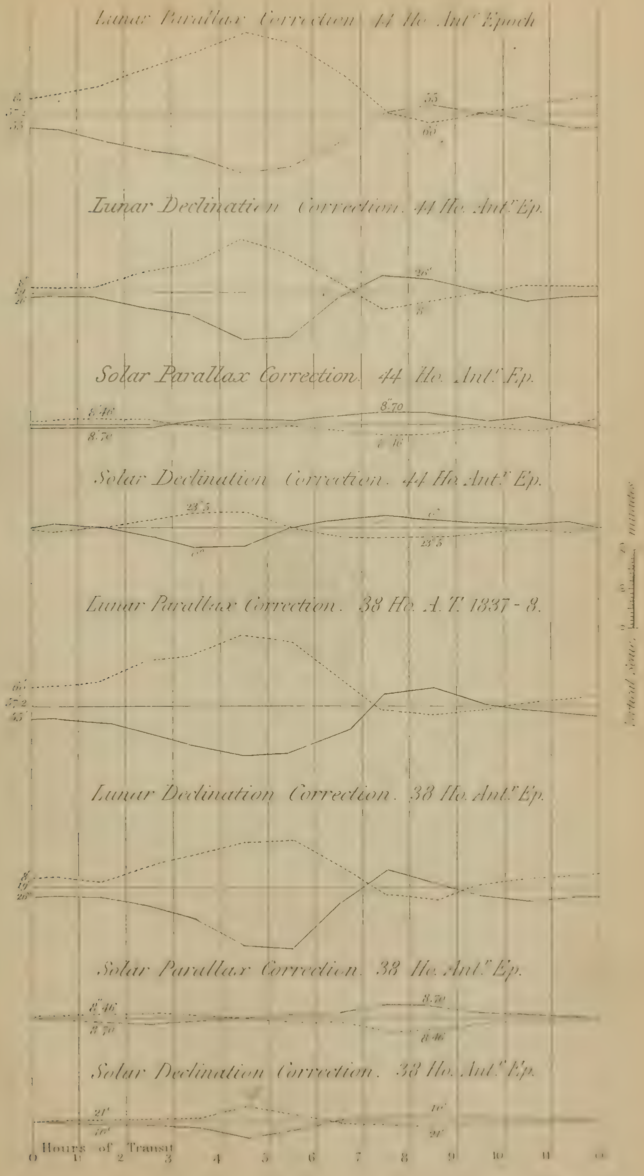

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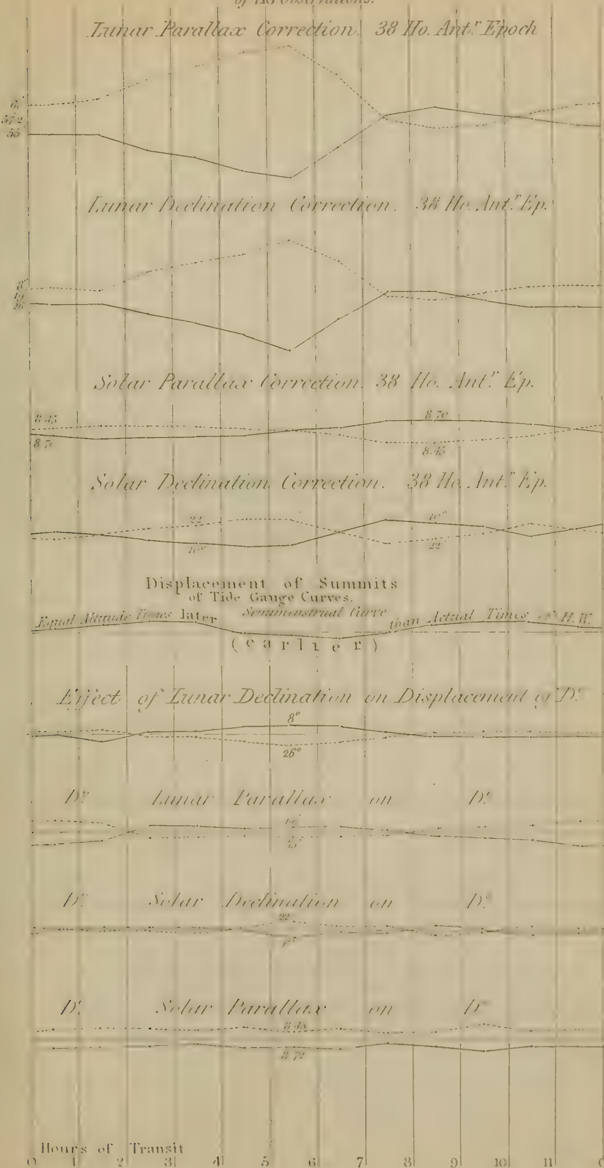

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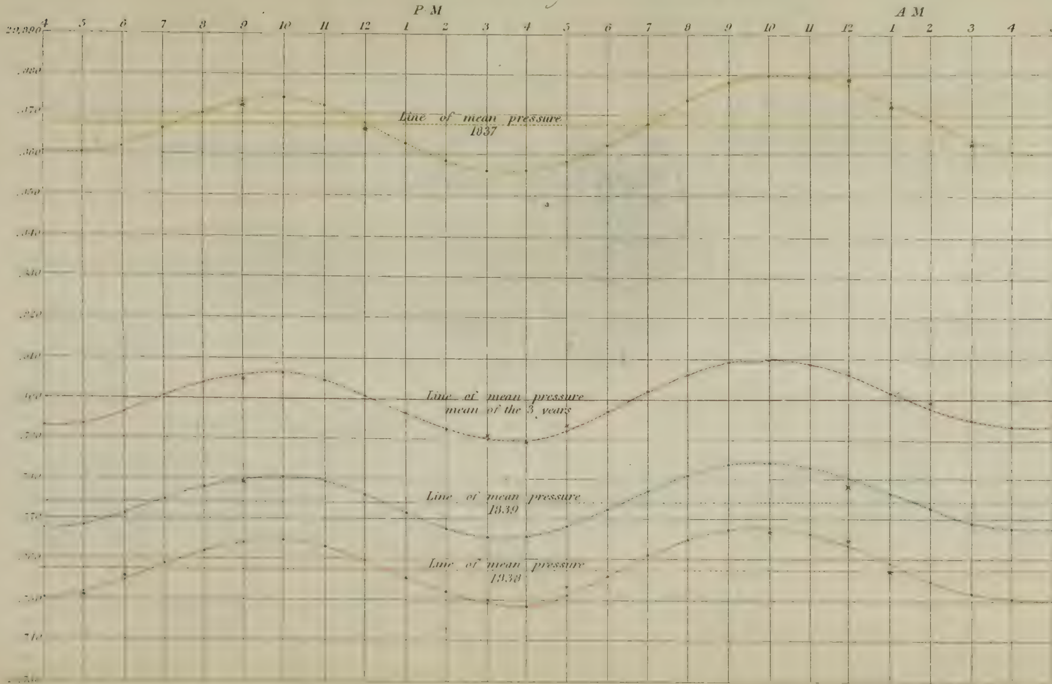
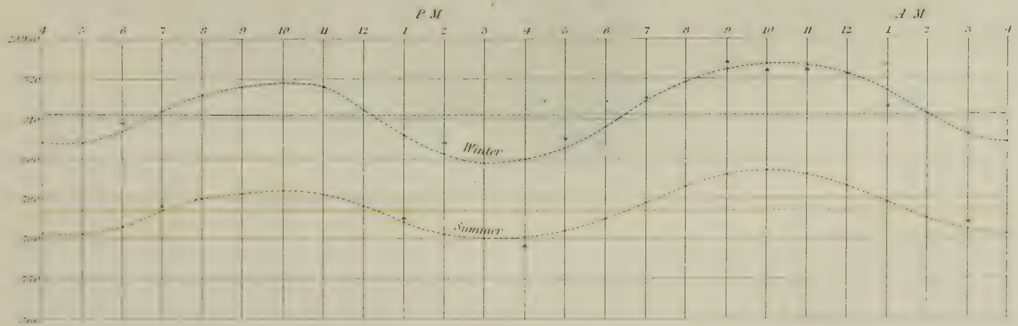
Shewing the mean hourly pressure for each of the years 1837 _38 _39 and the mean of the 3 years reduced to 32°.



Shewing the mean Hourly Pressure for the Periods of Winter & Summer each comprising 6 months.



Shewing the mean Hourly progression of the Barometer in the four Seasons of Spring, Summer, Autumn, & Winter.

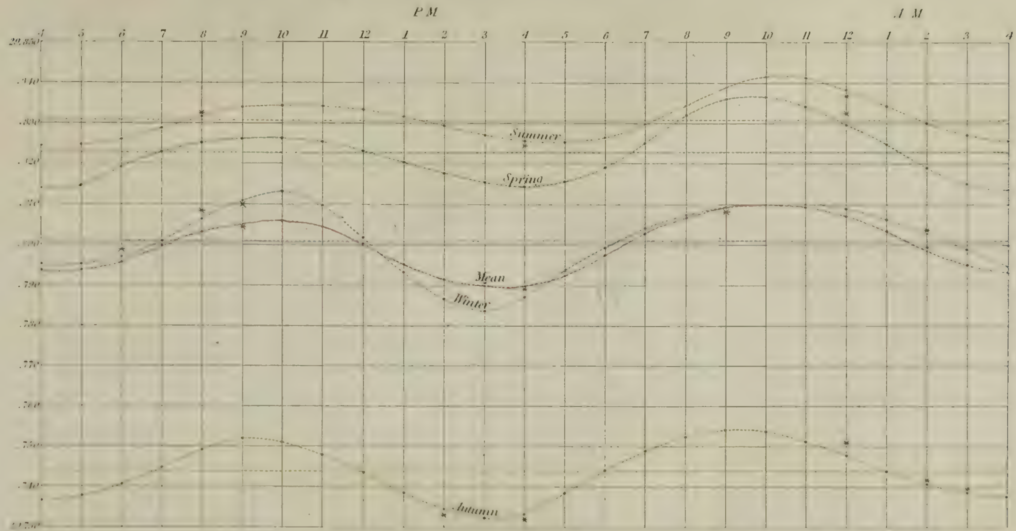


Plate 8, page 149.

Showing the mean Hourly Pressure, Thermometer, Hygrometer, and the Dew Point as deduced by Professor Apjohn's formula.